

EFFLUVIUM

BOYLE

1673



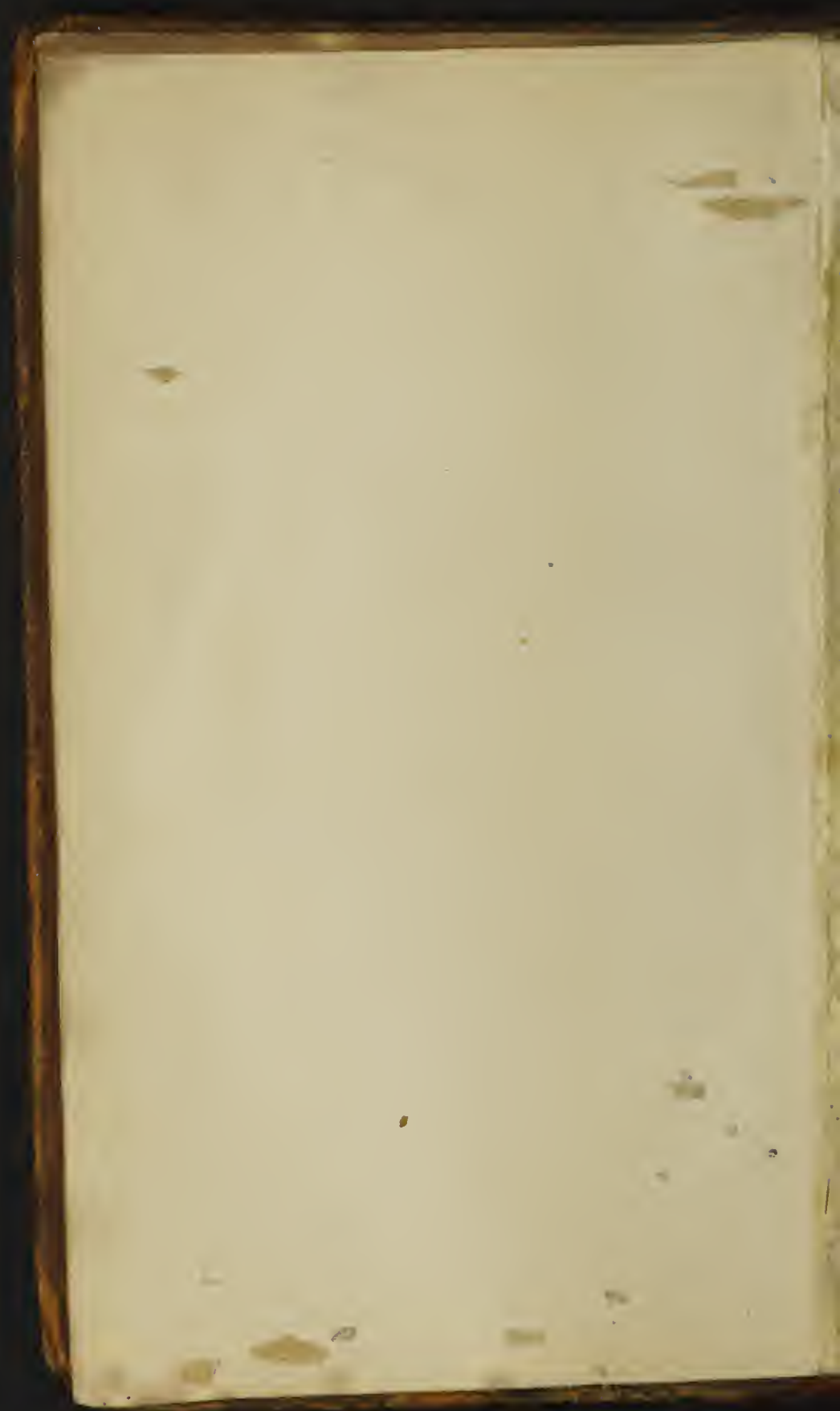




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OF THE
Strange SUBTILTY
OF
EFFLUVIUMS.

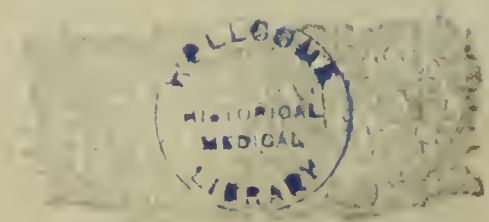
BY
The Honorable ROBERT BOYLE.



L O N D O N :

Printed by W. G. for *M. Pitt* at the sign of the
White Hart, over-against the little North
Door of *St Paul's Church*. 1673.

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OF
The strange SUBTILTY
OF
EFFLUVIUMS.

CHAP. I.



Hether we suppose with the Antient and Modern *Atomists*, that all sensible Bodies are made up of *Corpuscles*, not only insensible, but indivisible; or whether we think with the *Cartesians*, and (as many of that Party teach us) with *Aristotle*, that Matter, like Quantity, is indefinitely, if not infinitely divisible: It will be consonant enough

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to either Doctrine, that the *Effluvia* of Bodies may consist of Particles *extremely small*. For if we embrace the Opinion of *Aristotle* or *Des-Cartes*, there is no stop to be put to the subdivision of Matter, into Fragments, still lesser and lesser. And though the *Epicurean* Hypothesis admit not of such an *interminate* division of Matter, but will have it stop at certain solid Corpuscles, which for their not being further divisible are called *Atoms* ($\alpha\tau\omicron\mu\omicron\iota$;) yet the Assertors of these do justly think themselves injured, when they are charged with taking the *Motes* or small Dust, that fly up and down in the Sun-Beams, for their Atoms; since, according to these Philosophers, one of those little grains of Dust, that is visible only when it plays in the Sun-Beams, may be composed of a multitude of Atoms, and exceed many thousands of them in Bulk. This the Learned *Gassendus* in his Notes on *Diogenes Laertius* makes probable by the instance of a small Mite, which, though scarce distinctly

distinctly discernable by the naked Eye, unless when 'tis in motion; does yet in a good Microscope appear to be a compleat Animal, furnished with all necessary Parts; which I can easily allow, having often in Cheese-Mites very distinctly seen the Hair growing upon their Legs. And to the former Instance I might add, what I have elsewhere told you of a sort of Animals far lesser than Cheese-Mites themselves, namely those that may be often-times seen in Vinegar. But what has been already said may suffice for my present purpose, which is only to shew, that the wonderful minuteness I shall hereafter ascribe to *Effluvia*, is not inconsistent with the most received Theories of Naturalists. For otherwise in this Essay the Proofs I mean to employ, must be taken, not *à Priori*, but *à Posteriori*. And the Experiments and Observations I shall employ on this occasion will be chiefly those, that are referrible to one of the following Heads.

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I. The

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- I. *The strange Extensibility of some Bodies whilst their Parts yet remain tangible.*
- II. *The multitude of Visible Corpuscles, that may be afforded by a small portion of Matter.*
- III. *The smallness of the Pores at which the Effluvia of some Bodies will get in.*
- IV. *The small decrement of Bulk or weight, that a Body may suffer by parting with great store of Effluvia.*
- V. *The great quantity of Space that may be fill'd, as to sense, by a small quantity of Matter when rarified or dispers'd.*

But though to these distinct Heads I shall design distinct Chapters, yet

This Essay was designed to be but a part of the Author's Notes upon his Essay about Salt-peter.

you must not expect to find the Instances solicitously marshall'd, but set down in the order they occur'd to me; such a liberty being allowable

allowable in a Paper ; where I pretend not to write *Treatises* , but *Notes*

CHAP. II.

AMong many things that are gross enough to be the Objects of our Touch , and to be managed with our Hands , there are some that may help us to conceive a wonderful minuteness in the small Parts they consist of.

I do not remember what *Cardan*, and since him another *Writer* have deliver'd about the Thinness and Slenderness to which Gold may be brought. And therefore without positively assenting to , or absolutely rejecting what may have been said about it by others , I shall only borrow on this occasion, what I have mention'd on another upon my own Observation ; namely,

*In a Paper about
Improbable Truths.*

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That Silver, whose Ductility and Tractility are very much inferiour to those of Gold, was, by my procuring, drawn out to so slender a Wire, that, when we measur'd it, which was somewhat troublefom to do, with a long and accurate measure, we found, that eight Yards of it did not yet fully counterpoise one Grain: So that we might add a Grain more without making the Scale, wherein 'twas put, manifestly preponderate, notwithstanding the Tenderneſs of the Ballance. Whence we concluded, that a single Grain of this Wire amounted to 27 Foot, that is, 324 Inches. And ſince Experience informs us, that half an *English* Inch can by Diagonal Lines be divided into 100 parts great enough to be eaſily diſtinguiſh'd, even for Mechanical uſes, it follows, that a Grain of this wire-drawn Silver may be divided into 64800 parts, and yet each of theſe will be a true metalline, though but ſlender and ſhort, Cylinder, which we may very well conceive to conſiſt
yet

yet of a multitude of minuter parts. For though I could procure no Gilt Wire near so slender as our newly mention'd Silver-wire; yet I tryed that some which I had by me was small enough to make one Grain of it fourteen foot long: At which rate an Ounce did amount to a full Mile, consisting of 1000 Geometrical Paces, (of 5 foot a-piece,) and 720 foot over and above. And if now it be permitted to suppose the Wire to have been, as in probability it might have been, further drawn out to the same slenderness with the above-mention'd Silver-wire, the Instance will still be far more considerable; for in this case, each of those little Cylinders, of which 64800 go to the making of one Grain, will have a superficial *Area*, which, except at the *Basiss*, will be cover'd with a Case of Gold; which is not only separable from it by a mental Operation, but perhaps also by a Chymical one. For I remember, that from very slender gilt Wire, though I could get
 none

none so slender as this of meer Silver; I did more than once, for Curiosities sake, so get out the Silver; that the golden Films, whilst they were in a Liquor that plumpt them up, seem'd to be solid-wires of Gold: But when the Liquor was withdrawn, they appear'd; (as indeed they were) to be oblong and extremely thin and double Membranes of that Metal, which, with an Instrument that had been delicate enough, might have been ripp'd open, and displayed, and been made capable of further Divisions and Subdivisions. To this I shall add, that each of the little Silver Cylinders I lately spake of, must not only have its little *Area*; but its Solidity; and yet I saw no reason to doubt, but that it might be very possible, if the Artificer had been so skilful and willing as I wish'd, to have drawn the same quantity of Metal to a much greater length, since even an Animal substance is capable of being brought to a slenderness much surpassing that of our Wire, supposing the Truth
of

of an Observation of very credible Persons critical enough in making Experiments, which, for a Confirmation and an Improvement of our present Argument, I shall now subjoyn. An Ingenious Gentlewoman of my Acquaintance, Wife to a Learned Physician, taking much pleasure to keep Silk-worms, had once the Curiosity to draw out one of the Oval Cases, (which the Silk-worm spins, not, as 'tis commonly thought, out of its Belly, but out of the Mouth, whence I have taken pleasure to draw it out with my Fingers,) into all the Silken-wire it was made up of, which, to the great wonder as well of her Husband, as her self, who both inform'd me of it, appeared to be by measure a great deal above 300 Yards, and yet weigh'd but two Grains and a half: so that each Cylindrically shap'd Grain of Silk may well be reckon'd to be at least 120 Yards long.

Another way, I remember, I also employed to help men by the extensibility of Gold the better to conceive
the

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the Minuteness of the Parts of Solid Bodies.

We took six beaten Leaves of Gold, which we measured one by one with a Ruler purposely made for nice Experiments, and found them to have a greater equality in Dimensions, and to be nearer true Squares, than could be well expected: The side of the Square was in each of them exactly enough three Inches and $\frac{2}{3}$; (or $\frac{1}{4}$;) which number being reduc'd to a Decimal Fraction, viz. $\frac{3\frac{12}{100}}$, and multiplied by it self, affords $\frac{10\frac{1562}{10000}}$ for the *Area*, or superficial Content of each square Leaf: And this multiplied by 6, the number of the Leaves, amounts to $\frac{63\frac{1375}{10000}}$ square Inches, for the *Area* of the six Leaves. These being carefully weigh'd in a pair of tender Scales, amounted all of them to one Grain and a quarter: And so one Grain of this foliated Gold was extended to somewhat above fifty Inches; which differ'd but about a fifth part from an Experiment of the like nature, that I remember I made
many

many years ago in a pair of exact Scales; and so small a difference may very well be imputed to that of the pains and diligence of the Gold-Beaters, who do not always work with equal strength and skill, nor upon equally fine and ductile Gold.

Now if we recal to mind what I was lately saying of the actual divisibility of an Inch into an hundred sensible parts, and suppose an Inch so divided to be applied to each side of a square Inch of the Leaf-Gold newly mention'd, 'tis manifest that by subtle parallel Lines, drawn between all the opposite Points, a Grain of Gold must be divisible into five hundred thousand little Squares, very minute indeed, but yet discernible by a sufficiently sharp-sighted Eye. And if we suppose an Inch to be divided into two hundred parts, as I lately told you it was in a Ruler I employ, then, according to the newly recited way, the number of the Squares, into which a single Grain is capable of being divided, will amount to no less than two Millions. There

There is yet another way, that I took to shew that the extensibility, and consequently the divisibleness of Gold is probably far more wonderful, than by the lately mention'd Tryal it appears.

For this purpose I went to a great Refiner, whom I used to deal with for purify'd Gold and Silver, and inquir'd of him, how many Grains of Leaf-Gold he was wont to allow to an Ounce of Silver, when it was to be drawn into gilt Wire as slender as an Hair? To this he answer'd me, that eight Grains was the proportion he allowed to an Ounce when the Wire was to be well gilt; but if it were to be more slightly gilt, six Grains would serve the turn. And to the same purpose I was answer'd by a skilful Wire-drawer. And I remember, that desiring the Refiner to shew me an Ingot of Silver, as he did at first gild it; he shew'd me a good fair Cylindrical Bar, whereon the Leaf-Gold, that overlaid the surface, did not appear to be by odds

odds so thick as fine *Venetian* Paper; and yet comparing this with gilt Wire, which I also desired to see, the Wire appeared to be the better gilt of the two; possibly because the Gold in passing through the various Holes, was by the sides of them not only extended but polished, which made it look more vividly than the unpolish'd Leaves that gilded the Ingot.

So that, if we suppose an Ounce of the gilt Wire formerly mention'd to have been gilt with six Grains of Leaf-Gold, it will appear by an easie calculation, that at this rate one Ounce of Gold, employ'd on gilding Wire of that slenderness, would reach between ninety and an hundred Miles. But if now we further suppose, as we lately did, that the slender Silver-wire, mention'd at the beginning of this Chapter, were gilt; though we should allow it to have (because of its exceeding slenderness,) not, (as the former) 6 Grains, but 8 Grains of Leaf-Gold to an Ounce of Silver,

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it must be acknowledged, that an hollow Cylinder or sheath of Gold weighing but eight Grains, may be so stretch'd, that 'twill reach to no less than 60 times as much (in weight) of Silver-wire as it covers: [I said 60 times, for so often is 8 contain'd in 480, the number of Grains in an Ounce;] and consequently (a Grain of *that* Wire having been found to be 27 foot long,) the Ounce of Gold would reach to seven hundred seventy seven thousand six hundred foot; that is, an hundred fifty five Miles and above a half. And if we yet further suppose this superficial or hollow Cylinder of Gold to be slit all along, and cut into as slender lists or thongs as may be, we must not deny that Gold may be made to reach to a stupendious length. But we need not this last supposition to make what preceded it an amazing thing: which yet though it be indeed Stupendious and *seem* Incredible, ought not at all to be *judg'd* Impossible; being no more than what upon the

Suppo-

Suppositions and Observations above laid down, does evidently follow.

CHAP. III.

AFTER what has been said of the minuteness of *tangible* Objects, will be proper to subjoyn some instances of the smallness of such as yet continue *visible*. But in regard these Corpuscles are singly too little to have any common measure apply'd to any of them, we must make an estimate of their minuteness by the number of those into which a small portion or fragment of matter may be actually divided, the multitude of these being afforded by so inconsiderable a Quantity of matter, sufficiently declaring, that each of them, in particular, must be marvelously little.

Among the instances, where the smallness of Bodies may be deduc'd from what is immediately the Object of Sight, it may not be unfit to take

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notice

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notice of the evaporation of Water, which though it be granted to consist of gross particles in comparison of the spirituous and odoriferous ones of divers other Liquors, as of pure Spirit of Wine, Essential Oyls of Spices, &c. yet to shew that a small Quantity of it may be dispers'd into a multitude of manifestly visible Corpuscles, I thought upon, and more than once try'd, the rarefaction of it into Vapors by help of an *Æolipile*, wherein, when I made the Experiment the last time, I took the pains to register the Event as follows.

We put an Ounce of common Water into an *Æolipile*, and having put it upon a Chafing-dish of coals, we observ'd the time when the streams of Vapors began to be manifest. This stream was for a good while impetuous enough, as appear'd by the noise it made, which would be much increased, if we applied to it at a convenient distance a kindled brand, in which it would blow up
the

the fire very vehemently. The stream continued about a quarter of an hour (sixteen minutes or better,) but afterwards the Wind had pauses and gusts for two or three minutes before it quite ceased. And by reason of the shape of the *Æolipile*, (which being fram'd chiefly for other purposes; was not so convenient for this) a great portion of the Vapors condens'd in the upper part of it, and fell down in drops; so that supposing that they also had come out in the form of Wind, and the blast had not been intermitted toward the latter end, I guess'd it might have continued uninterruptedly 18 or 20 minutes.

Note, That applying a measure to the Smoak, that came out very visible in a form almost conical, where it seem'd to have an Inch or more in Diameter, 'twas distant from the hole of the *Æolipile* about twenty Inches; and five or six Inches beyond that, though it were spread so much, as to have four or five Inches in Diameter, yet the not uniform

but still-cohering Clouds (which was the form wherein the Vapors appear'd) were manifest and conspicuous.

After the rarefaction of Water when 'tis turn'd into Vapors ; we may consider *that* of Fewel when 'tis turn'd into Flame ; to which purpose I might here propose several Tryals as well of our own as others ; about the prodigious Expansion of some Inflammable Bodies upon their being actually turn'd into Flame. But in this place to mention all these ; would perhaps too much intrench upon another Paper ; and therefore I shall here propose to your consideration but one instance ; and that very easie to be tryed ; of which I find this account among my *Advertisaria*.

Having oftentimes burnt Spirit of Wine ; and also Oyl in Glass-lamps ; that for certain uses were so made ; that the surface of the Liquor was still circular ; 'twas obvious to observe ; how little the Liquor would
sub-

subside by the waſt that was made of it, in about half a quarter of an hour. And yet if we conſider, that the naked Eye after ſome Exerciſe, may, as I have often tryed, diſcern the motions of a *Pendulum*, that ſwings faſt enough to divide a ſingle minute of an hour into 240 parts, and conſequently half a quarter of an hour into 1800 parts; if we alſo conſider into how many parts of the time imployed by a *Pendulum*, the Vibrations, ſlow enough to be diſcernible by the Eye, may be mentally ſubdivided; and if we further conſider, that without intermiſſion, the Oyl is preyed upon by an actual Flame, and the particles of it do continually furniſh a conſiderable ſtream of ſhining matter, that with a ſtrange celerity is always flying away; we may very well conceive, that thoſe parts of Flame into which the Oyl is turned, are ſtupendiouſly minute, ſince, though the waſting of the Oyl is in its progreſs too ſlow to be perceived by the Eye, yet 'tis

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undoubted that there is a continual decrement of the depth of the Oyl, the Physical surfaces whereof are continually and successively attenuated and turn'd into flame; and the strange subtilty of the Corpuscles of flame would be much the stronglier argued, if we should suppose, that instead of common Oyl the flame were nourish'd by a fewel so much more compact and durable, as is that inflammable substance made of a Metalline Body, of whose lastingness

*In some Papers
about Flame.*

I have elsewhere made particular mention, after having taught the way of preparing it.

Having in a pair of tender Scales carefully weigh'd out half a Grain of good Gunpowder, we laid it on a piece of Tile, and whelm'd over it a vessel of glass (elsewhere describ'd, and often mention'd) with a Brass-plate to cover the upper orifice of it. Then having fir'd the Gunpowder, we observ'd that the smoak of it did opacate, and as to sense so fill the whole

whole cavity of the Glass, though its *Basis* were eight inches, its perpendicular height above twenty inches, and its figure far more capacious than if it were conical; and this smoak, not containing it self within the vessel, issued out at two or three little intervals, that were purposely left between the orifice of the vessel and the plate that lay upon it. This cover we then remov'd, that we might observe how long the smoak would continue to ascend; which we found it would do for about half a quarter of an hour, and during near half that time, (*viz.* the three first minutes) the continually ascending smoak seem'd to be, at its going out, of the same Diameter with the orifice at which it issu'd; and it would ascend sometimes a foot, sometimes half a yard, sometimes two foot or more into the Air, before it would disperse and vanish into it.

Now if we consider, that the cavity of this round Orifice was two inches in Diameter, how many my-

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riads of visible Corpuscles may we easily conceive throng'd out at so large an out-let in the time above-mention'd, since they were continually thrusting one another forwards? And into so many visible Particles of smoak must we admit, that the half Grain of Powder was shatter'd, beside those multitudes, which, having been turn'd into actual flame, may probably be suppos'd to have suffer'd a comminution, that made them become invisible. And though I shall not attempt so hopeless a work, as to compute the number of these small Particles, yet to make an estimate whereby it would appear to be exceeding great, I thought fit to consider; how great the Proportion was between the spaces, that to the Eye appear'd all full of smoak, and the dimensions of the Powder that was resolv'd into that smoak. Causing then the Glass to be fill'd with common Water, we found it to contain above two and twenty Pints of that liquor, and causing one of those
measures

measures to be weigh'd, it was found to weigh so near a pound (of sixteen ounces,) that the computation of the whole Water amounted to at least 160000 grains, and consequently 320000 half grains. To which if we add, that this Gunpowder would readily sink to the bottom of Water, as being (by reason of the Saltpeter and Brimstone, that make up at least six parts of seven of it) *in specie* heavier than it, and in likelihood twice as heavy, (for 'tis not easie to determine it exactly,) we may probably guess the space to which the smoak reach'd to exceed 500000 times that, which contain'd the unfir'd Powder; and this, though the smoak, being confin'd in the vessel, was thereby kept from diffusing it self so far as by its streaming out it seem'd likely that it would have done.

To these Instances from Inanimate Bodies, I shall subjoyn one more taken from Animals. Whereas then men have with Reason wonder'd, that so small a Body as a Cheese-mite, which

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which by the naked Eye is oftentimes not to be taken notice of, unless it move, (if even then it be so,) should by the Microscope appear to be an Animal furnish'd with all necessary parts; whereas this, I say, has given just occasion to conclude, that the Corpuscles that make up the parts of so small an Animal, must themselves be extremely small; I think the Argument may be much improved by the following Consideration. Those that have had the Curiosity to open from time to time Eggs that are sat upon by a hatching Hen cannot but have observed, how small a proportion in reference to the bulk of the whole Egg the Chick bears; when that, which the Excellent *Harvey* calls *Punctum saliens*, discloses the motion of the Heart, and the colour of the Blood; and that even about the seventh or eighth day the whole Chick now visibly form'd, bears no great proportion to the whole Egg, which is to supply it with Aliment, not only for its nourishment,

ment, but speedy growth for many days after.

To apply this now to the matter in hand, having several times observed and shewn to others, that Cheese-mites themselves are generated of Eggs, if we conceive, that in these Eggs, as in ordinary ones, the Animal at its first formation bears but a small proportion to the bulk of the whole Egg, the remaining part being to suffice for the food and growth of the *Embryo* probably for a pretty while; since, if an Ingenious person, that I desired to watch them, did not mis-inform me, they used to be about ten or twelve days in hatching; this whole Egg it self will be allowed to be but little in reference to the Mite it came from, how extremely and unimaginably minute may we suppose those parts to be; that make up the Alimental Liquors, and even the Spirits, that passing through the Nerves or Analogous parts, serve to move the Limbs and Sensories of but, as it were,
the

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the Model of such an Animal, as when it rests, would not (perhaps) itself to the naked Eye be so much as visible; and in which we may presume the nobler sort of stable parts to be of an amazing slenderness; if we consider, that, though in other hairy Animals, the Optick or some other of the larger Nerves do I know not how many times, in thickness and circuit surpass a hair of the same Animal; yet in a Cheese Mite, though none of the largest of those Creatures; we have divers times manifestly seen, as is before intimated, single Hairs that grow upon the Legs.

Another way there is, that I'mployed to give men cause to think that the invisible *Effluvia* of Bodies that wander through the Air may be strangely minute; and this was by shewing how small a fragment of matter may be resolved into particles minute enough to associate themselves in such numbers with a Fluid so much more dense than Air, as
Water

Water is, as to impart a determinate Colour to the whole liquor. What I did with *Cocheneel* in prosecution of this design, my Experiments about Colours may inform you; but I shall now relate the success of an attempt made another way, for which perhaps some of your friends the Chymists will thank me; though I was not solicitous to carry on the Experiment very far with Gold, not because I judged that less divisible into a number of colour'd particles, but because I found, as I expected, that the paleness of the native colour of the Gold may make it in the end less conspicuous, though, if I had then had by me a *Menstruum*, as I sometimes had; that would dissolve Gold blood-red; perhaps the experiment with Gold would have surpass'd that, which 'tis now time I should begin to relate; as soon as I have hinted to you by the way, that, for varieties sake, I made a tryal with Copper calcin'd *per se*, that I might not be accused of having omitted to employ

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employ a Metal whose Body Chymists suppose to be much opened by Calcination. And though the event were notable even in Comparison of that of the experiment made with *Cocheneel*, yet my conjectures inclin'd me much to prefer the way describ'd in the following Account.

We carefully weigh'd out in a pair of tender Scales one grain of Copper not-calcin'd, but barely fil'd; and because, as we made choice of this Metal for its yielding in most *Menstruums* a Blew, which is a deep and conspicuous colour; we also chose to make a solution, not in *Aqua fortis* or *Aqua regis*, but the Spirit of *Sal Armoniack* (as that is an urinous Spirit,) having found by former tryals, that this *Menstruum* would give a far deeper solution than either of the others. This lovely Liquor, of which we us'd a good proportion, that all the Copper might be thoroughly dissolved, we put into a tall cylindrical Glass of about four inches in Diameter, and by degrees pour'd to it of distill'd Water,

Water; which is more proper in this case than common Water, which has oftentimes an inconvenient Saltness, 'till we had almost fill'd the Glass, and saw the colour grow somewhat pale, without being too dilute to be manifest; and then we warily pour'd this liquor into a conical Glass, that it might be the more easie to fill the vessel several times to the same height. This conical Glass we filled to a certain mark four times consecutively, weighing it, and the liquor too, as often in a pair of excellent Scales purposely made for Statical experiments, and which, though strong enough to weigh some pounds in each Scale, would, when not too much loaden, turn with about one grain. These several weights of the Glass, together with the contained liquor, we added together, and then carefully weighing the empty Glass again, we deducted four times its weight from the above-mentioned sum, and thereby found the weight of the liquor alone, to be that, which
reduc'd

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reduc'd to grains amounted to 28534; so that a grain of Copper, which is not full half so heavy *in specie* as fine Gold, communicated a Tincture to 28534 times its weight.

But now if you please to take notice, that the scope of my Experiment was to shew, into what a number of parts one grain of Copper might be divided; you will allow me to consider, as I did, that this multitude of parts must be estimated by the Proportion, not so much in weight as in bulk, of the tinging Metal to the tinged Liquor, and consequently, since that divers Hydrostatical tryals have inform'd me, that the weight of Copper to the weight of Water of the same bulk is *proximè* as 9 to 1, a grain-weight of Copper is in bigness but the ninth part of as much Water as weighs a grain; and so the formerly mention'd number of the grains of Water must be multiplied by 9, to give us the Proportion between the tinging and tinged Bodies, that is, that a single grain of
Copper

Copper gave a blewness to above 256806 parts of limpid Water, each of them as big as it. Which, though it may seem stupendious, and scarce credible; yet I thought fit to prosecute the Experiment somewhat farther, by pouring all the liquor out of the tall cylindrical Glass into another clean vessel, whence filling the conical Glass twice, and emptying it as often into the same cylindrical Glass, the third time I fill'd the conical Glass with colourless distill'd Water, and pouring that also into the cylindrical Glass, we found the mixt liquor to have yet a manifest, though but a pale, blewness. And, lastly, throwing away what was in the cylindrical Glass, we poured into it, out of the same conical Glass, equal parts of distill'd colourless Water, and of the tincted Liquor we had formerly set apart in the clean Vessel, and found, that, though the colour were very faint and dilute, yet an attentive Eye could easily discern it to be blewish; and so it was judg'd

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by

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by an intelligent Stranger that was brought in to look upon it, and was desir'd to discover of what colour he thought it to be. Whereby it appears, that one grain of Copper was able to impart a colour to above double the quantity of Water above mentioned.

This Experiment I have allow'd my self to be the longer and more particular in relating, both because I know not, that any such has been hitherto either made or attempted, and because it will probably gratifie your Chymists, that love to have the Tinctures of Metals believ'd very diffusive; and because, if Circumstances were not added, it would seem to you as well incredible; as perhaps it does seem stupendious, that a portion of matter should be able to impart a *conspicuous* colour to above 256806 times its bulk of Water, and a manifest tincture to above 385200, (for so it did, when the proportion of the ting'd part to the whole mixture, made of it and the unting'd part,

part, was as 2 to 31,) and a faint, but yet *discernible* and distinguishable colour to above five hundred and thirteen thousand six hundred and twenty times its bulk of Water.

CHAP. IV.

IT were easie for me (*Pyroph.*) to give you several Instances, to shew, that the *Effluvia* of *Liquors* may get in at the Pores of Bodies that are reputed of a close Texture, but I shall at present forbear to mention such Examples, not only because they belong to another place*, where I take notice of them, but because many such would not seem so remarkable, nor be so considerable to our present purpose, as a few taken from Bodies that are *not Fluid*.

* A Discourse of Pores of Bodies, and Figures of Corpuscles.

And first, it is deliver'd by Writers
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of good credit, that several Persons, (for the Experiment does not hold in all) by barely holding for some time dried *Cantharides* in their hands, have been put to much pain at the neck of the Bladder, and have had some other parts ministring to the secretion of Urine sensibly injured. That this is true, I am induced to believe, by what I have elsewhere related to you of the unwelcome experiment I had of the effect of *Cantharides* applied but outwardly to my neck, and that unknown to me, upon the Urinary Passages; and that these Operations are due to material Effluxes, which, to get into the Mass of Blood, must pass through the pores of the skin, you will not, I presume, put me to prove.

Scaliger Exercit. 186. relates, that in *Gascony*, his Countrey, there are Spiders of that virulency, that, if a man treads upon them to crush them, their poyson will pass through the very soles of his Shooes. Which story, notwithstanding the Reputation of
of

of the Author, I should perhaps have left unmention'd, because of a much stranger about Spiders, which he relates in the same Section, but that I met with one that is analogous in the diligent *Piso's* late History of *Brafile*; where, having spoken of another venomous Fish of that Country, and the Antidotes he had successfully used to cure the hurts it inflicts, he proceeds to that Fish the Natives call *Amoreatim*, of one kind whereof, call'd by the *Portugals* *Peize Sola*, his words are these; *Quæ mira sanè efficacia non solum manum vel levissimo attactu, sed & pedem, licet optimè calcatum, Piscatoris incautè pisciculum conterentis, Paralyti & Stupore afficit, instar Torpedinis Europæ, sed minus durabili.* Lib. 5. cap. 14.

What I shall ere long have occasion to tell you of the power of the *Torpedo*, and some other Animals, to affect the Hand and Arm of him that strikes them, seems applicable to the matter under consideration: For, though their affecting the striker at

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a distance, may very well be ascrib'd to the stupefactive or other venomous Exhalations that expire (and perhaps are as it were darted) from the Animal irritated by the stroke, and are breath'd in together with the air they infect; yet their benumbing, or otherwise affecting the Arm that struck them, rather than any other part, seems to argue, that the poysonous steams get in at the pores of the skin of the Limb, and so stupifie, or otherwise injure, the nervous and musculous parts of it.

Other Examples belonging to this Section may be referr'd hither from divers other places in these Papers about Occult Qualities, and therefore I shall only add here that most remarkable Proof, *That some Emanations, even of solid Bodies, may be subtil enough to get through the pores, even of the closest Bodies*; which is afforded us by the *Effluvia* of the Loadstone, which are by Magnetical Writers said to penetrate without resistance all kind of Bodies. And though

though I have not tryed this in all
 forts, yet having tryed it in Metals
 themselves, I am apt to think, the
 general Rule admits of very few Ex-
 ceptions, especially, if that can be
 fully made out, which is affirm'd
 about the perviousness of Glass to
 the Effluxions of the Loadstone.
 For, not only Glass is generally re-
 puted to be as close a Body as any is,
 but (which weighs more with me)
 I have by Tryals purposely made,
 had occasion to admire the closeness
 of very thin pieces of Glass. But
 the reason why I just now express'd
 my self with an *If*, was, because I
 was not entirely satisfied with the
 Proof wont to be acquiesc'd in, of
 the perviousness of Glass; namely,
 that in Dials and Sea-Compasses that
 are cover'd with plates of Glass, the
 Needle may be readily moved to and
 fro by a Loadstone held over it. For
 these Plates being commonly but
 fasten'd on with Wax, or at best
 with Cement, a Sceptick may pre-
 tend, that the magnetical *Effluvia*

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pass not through the Glass, but through that much more pervious matter, that is imployed to secure the Commissures, only from the access of the Air. To put then the matter past doubt, I caused some Needles to be Hermetically seal'd up in Glass pipes, which being laid upon the surface of water (whereon by reason of the bigness of the Cavities they would lightly float,) the included Needles did not only readily feel the virtue of an externally applied Loadstone, (though but a weak one) but complied with it so well, that I could easily, by the help of the Needle, lead, without touching it, the whole Pipe, this was shut up in, to what part of the surface of the water I pleased. And I also found, that by applying a better Loadstone to the upper part of a sealed Pipe, and a Needle in it, I could make the Needle leap up from the lower part as near to the Loadstone as the interposed Glass would give it leave.

But I thought it would be more considerable, to manifest that the
Magne-

Magnetical *Effluvia*, even of such a dull Body, as the Globe of the Earth, would also penetrate Glass. And though this seem difficult to be tryed, because no ordinary Loadstone, nor any Iron touch'd by it, was to be employed to work on the included Iron; yet I thought fit to attempt it after this manner: I took a cylindrical piece of Iron of about the bigness of ones little finger, and between half a foot and a foot long, (for I had formerly observed, that the quantity of unexcited Iron furthurs its Operation upon excited Needles,) and having Hermetically seal'd it up in a Glass-pipe but very little longer than it; I supposed, that if I held it in a perpendicular posture, the Magnetical *Effluvia* of the Earth, penetrating the Glass, would make the lower extreme of the Iron answerable to the North Pole; and therefore having applied this to the point of the Needle in a Dial, or Sea-Compass, that look'd toward the North, (for Authors mean not all the same thing by
the

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the Northern Pole of a Needle or Loadstone,) I presum'd it would, according to the Laws Magnetical (elsewhere mention'd) drive it away, which accordingly it did. And having for farther tryal inverted the included Iron, (so that the end which was formerly the lowermost, was now the uppermost) and held it in a perpendicular posture just under the same point of the Needle, that extreme of the Iron-rod, which before had driven away this point, being by this inversion become (in a manner) a South-Pole, did (according to the same Laws) attract it: By which sudden change of Poles, meerly upon the change of situation, it also appear'd, that the Iron ow'd its Virtue only to the Magnetism of the Earth, not that of another Loadstone, which would not have been thus easily alterable. And this Experiment I the more particularly relate, because this is not the only place, where I have occasion to make use of it.

CHAP.

CHAP. V.

ANOTHER proof of the great Subtlety of *Effluvia*, may be taken from the small Decrement of weight or bulk that a Body may suffer by parting with great store of such Emanations.

That Bodies, which infus'd in Liquors impregnate them with new Qualities suitable to those of the immers'd Bodies, do so by imparting to them somewhat of their own Substance, will, I presume, be readily granted by those that conceive not; how one Body should communicate to another a solitary and naked Quality, unaccompanied by any thing Corporeal to support and convey it. But I would not have you think, *Pyrophilus*, that the only matter of fact I have to countenance this notion, is that Experiment, which has convinc'd divers Chymists and Physicians, otherwise
not

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not friends to the Corpuscular Philosophy, that Medicines may operate without any consumption of themselves. For, *though* divers of these, some of them Learned men, have confidently written, that *Glass of Antimony* and *Crocus Metallorum*, being either of them infus'd in a great proportion of Wine, will make it vomitive; and if that liquor be poured off, and new be poured on, every new portion of such liquor will be impregnated with the same virtue, and this though the liquor be chang'd a thousand times, and yet the *Antimonial Glass* or *Crocus* will continue the same as well in weight as virtue; and *though* thence some of them, especially Chymists, argue, that some Metals without imparting any thing substantial, but only, as *Helmont* speaks of some of his *Arcana*, by irradiation: Yet, I confess, I have some doubts, whether the Experiment have been competently tryed, and shall not fully acquiesce in what has been said, till some skillful Experimenter deliver it upon his own

own Tryal, and acquaint us too, with what Instruments and what Circumpection he made it. For, besides that the Ingeniouest Physicians I have question'd about it, acknowledg'd the Taſt, and ſometimes the Colour of the Wine to be alter'd by the infus'd Mineral, I could not acquieſce in the affirmation of an ordinary Chymiſt or Apothecary, or even Phyſician, if he ſhould barely averr, that he had weigh'd an Antimonial Medicine before 'twas put to infuſe, and after the infuſion ended, and obſerv'd no decrement of weight. For I have had too much experience (as I elſewhere mention) of the difficulty of making exact Statical tryals; not to know, that ſuch Scales, as are wont to be imployed by Chymiſts and Apothecaries in weighing Drugs, are by no means fit to make tryals with the nicety which that I am ſpeaking of requires: It being eaſie, even with the better ſort of ſuch unaccurate Scales, eſpecially if they be not ſuſpended from ſome fixt thing,

but

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but held with the hand, to mistake half a grain or a grain; and perhaps a greater quantity, and at least more than by divers of the Experiments of this Essay appears necessary to be spent upon the impregnating of a considerable proportion of Liquor with Corporeal Effluxions. Beside that if, when the beaten *Crocus* of *Glass* be taken out of the Wine to be weigh'd again, the Experimenter be not cautious enough to make allowance for the Liquor that will adhere to the Medicament, 'tis plain that he may take notice of no decrement of weight, though there may be really *Effluviiums* of the Mineral amounting to several grains, imbib'd by the Liquor. And though he be aware of this, and dry the powder, yet 'tis not so easie, even for a skilful man to be sure that none of the more viscus particles of the Liquor stick to the Mineral, and being sensible upon the Ballance, though not to the Eye or Hand, repair the recess of those emetick Corpuscles that diffus'd them

themselves into the *Menstruum*. And
 the sense of these difficulties put me
 upon the attempting to make so no-
 ble an Experiment with excellent
 Scales, and the care that it deserves;
 But after a long tryal, an unlucky
 accident frustrated at last my endea-
 vours. But though, till competent
 Relators give us an account of this
 matter upon their own tryal, and
 repeat the Infusion very much oftener,
 than; for ought I find, any man has
 yet done, I must not acquiesce in all
 that is said of the Impregnation of
 Wine or other Liquors by *Antimonial*
Glass and *Crocus Metallorum*; yet that
 after divers repeated Infusions the
 Mineral substance should not be sen-
 sibly diminish'd in bulk or virtue,
 may well suffice to make this In-
 stance, though not the only or chief
 that may be brought for our pur-
 pose, yet a pertinent one to it. For
 that there is a powerful Emetick Qua-
 lity imparted to the Liquor, is ma-
 nifest by experience; and that the
 Mineral does not impart this virtue
 as

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as 'twere by irradiation, but by substantial effluxion, seems to me very probable; not only because I conceive not, how this can be done otherwise but because, as 'tis noted above, the Wine does oftentimes change colour by being kept a competent time upon the Mineral, as if it drew thence a Tincture; and even when it is not discolour'd, I think it unsafe to conclude, that the *Menstruum* has not wrought upon it. For I have kept good Spirit of Vinegar for a considerable time upon finely powder'd Glass of Antimony made *per se*, without finding the Spirit to be at all ting'd, though 'tis known, that Antimonial Glass is soluble in Spirit of Vinegar, as mine afterwards appear'd to be, by a longer digestion in the same Liquor. But there may be a great number of minute particles dissolved in the *Menstruum* before they be numerous enough to change the Colour of it. And with this agrees very well what is observ'd, That though too great a quantity of the pre-

prepar'd Antimony be put into the liquor, yet it will not be thereby made too strongly Emetick. For the Wine, being a *Menstruum*, will, like other *Menstruums*, be impregnated but to a certain measure, without dissolving the overplus of the matter that is put into it. And *Mars*, which is a harder and heavier body than Glass of Antimony, is it self in part soluble in good *Rhenish* or other white Wine, (and that in no long time,) and sometimes even in Water. YOUNG

I do not therefore reject the Emetick Infusion, as unfit to have a place in this Chapter, but till the experiment have been a little more accurately made, I think it inferior, as to our purpose, to some of the Instances to be met with in the next Chapter, and perhaps I also to that mention'd by *Helmont*, and tryed by more than one of my Acquaintance, concerning the Virtue of killing Worms, that Mercury imparts to the water or wine wherein it has been long enough infus'd, or else for

D a while

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a while decocted. Though Quick silver given in substance is commended as an effectual Medicine against

Worms, not only by many profest * Spagyristes, but by divers

* As *Quercetanus, Libavius, Zabata, Burggravius.*

** As *Vidius, Paracelsus, Casalpini, &c.*

** Methodists of good

Note. And though some other things, Chymical and Philosophical, keep me from being of their opinion, who think that in this case the Mercury impregnates the liquor as it were by Irradiation, rather than in a Corporeal manner, yet the Eye does not perceive, that even limpid water takes any thing from clean and well purg'd Mercury. which we know that divers corrosive liquors themselves will not work upon.

To this Instance I must add one that is yet freer from exceptions, which is, that having for Curiosity sake suspended in a pair of exact Scales, that would turn with a very small part of a grain, a piece of *Amber-greece* bigger than a Walnut, and

and weighing betwixt an hundred and six-score grains, I could not in three days and a half that I had opportunity to make the tryal, discover, even upon that Ballance, any decrement of weight in the *Amber-greece*; though so rich a perfume, lying in the open Air, was like in that time to have parted with good store of odoriferous Streams. And a while after suspending a Lump of *Assa fatida* five days and a half, I found it not to have sustain'd any discernible loss of weight, though, in spite of the unfavourable cold weather, it had about it a neighbouring Atmosphere replenish'd with foetid exhalations. And when twelve or fourteen hours after, perhaps upon some change of weather, I came to look upon it, though I found that in that time the *Æquilibrium* was somewhat alter'd, yet the whole Lump had not lost half a quarter of a grain; which induc'd me to think, that there may perhaps be Steams discernible even by our Nostrils, that are far more subtil than the odorous

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exhalations of Spices themselves. For, having in very good Scales suspended in the Month of *March* an ounce of Nutmegs, it lost in about six days five grains and a half. And an ounce of Cloves in the same time lost seven grains and five eighths.

You will perhaps wonder, why I do not prefer to the Instances I make mention of in this Chapter, that which may be afforded by the Loadstone, that is acknowledg'd continually to emit multitudes of Magnetical Steams without decrement of weight. But though I have not thought fit to pass this wholly under silence; yet I forbear to lay so much stress on it, not only because my Ballances have not yet satisfied me about the *Effluvia* of Loadstones, (for I take them not all to be equally diffusive of their Particles;) but because I foresee it may be doubted, *whether* Loadstones; like odorous Bodies, do furnish afresh of their own, all the Corpuscles that from time to time issue from them? Or, whether they

they be not continually repaired, *partly* by the return of the Magnetical Particles to one Pole that sallied out of the other; and *partly* by the continued passage of Magnetical matter (supplied by the Earth or other Mundane Bodies) it make the Pores or Channels of the Loadstone their constant Thorow-fares.

I doubt not but it will make it more probable, that a small Quantity of matter being scatter'd into invisible *Effluvia* may be exceedingly rarified and expanded, if it can be made appear, that this little portion of matter shall, for a considerable time, emit multitudes of visible parts, and that in so close an order among themselves, as to seem in their Aggregate but one intire liquor, endow'd with a stream-like motion; and a distinct *superficies*, wherein no interruption is to be seen, even by an Eye plac'd near it. To devise this Experiment, I was induc'd, by considering, that hitherto all the (total) dissolutions that have been made of

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Pigments, have been in liquors naturally cold, and consisting probably of much less subtile, and certainly of much less agitated parts, than that fluid aggregate of shining matter that we call *Flame*; whereas I argued, that if one could totally dissolve a Body compos'd of parts so minute as those of a Metal into actual *Flame*, and husband its *Flame* so, as that it should not immoderately waste, I should thereby dissolve the Metal in a far more subtil *Menstruum* than our common water, or *Aqua fortis*, or *Aqua Regis*, or any other known *Menstruum* I have yet imployed. And consequently the attenuation and expansion of the Metal in this truly Igneous *Menstruum* would much surpass not only what happens in ordinary Metalline solutions, but possibly also what I have noted in the third Chapter of this *Essay*, about the strange diffusion of Copper dissolv'd in Spirit of Urine and Water. In prosecution of this design, I so prepar'd one single grain of that
Metal,

Metal, by a way that I elsewhere teach, that it was dissolv'd in about a spoonful of an appropriated *Mentruum*. And then having caus'd a small Glass-lamp to be purposely blown to contain this liquor, and fitted it with a socket and wieck, we lighted the Lamp, which, without consuming the wieck, burnt with a flame large enough and very hot, and seem'd to be all the while of a greenish blew, as if it were a but finer and shining solution of Copper. And yet this one grain of prepar'd Metal ting'd the flame that was from moment to moment produc'd, during no less than half an hour and six minutes. And now if we consider, that in this flame there was an uninterrupted Succession of multitudes of colour'd Particles newly extricated, and flying off in every of those many parts wherein a minute of time may either actually or mentally be divided; and, if we consider Flame as a light and very agitated body, passing with a stream upwards through the Air,

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and if we also consider the quantity of liquor that would (as I shall by and by tell you) run through a Pipe of a much lesser diameter than that Flame; within the compass of the forementioned time: What a quantity of the streaming fluid we call Flame, if it could have been preserv'd and collected into one Body, may we suppose would appear to have issued out of one grain of Copper in the space of thirty-six minutes; and what a multitude of metalline Corpuscles may we suppose to have been supplied for the tinging of that Flame during so long a time? since a Cylindrical stream of water falling but through a very short Pipe of glass, constantly supplied with liquors, did pass at such a rate, that, though the aqueous Cylinder seem'd more slender by half, (or perhaps by two thirds or better) than the Flame, yet we estimated, by the help of a Minute-watch and a good pair of Scales; that, if I had had conveniencies to let it run long enough, the water efflux'd in thirty-
fix

ix minutes (the time of the Flames duration) would have amounted to above nine gallons, or, (reckoning pint of water to contain a pound of sixteen ounces) seventy-two pounds.

CHAP. VI.

THE last sort of Instances I shall propose to shew the strange subtilty of *Effluvia*, is of such, as discover the great quantity of space that may by a small quantity of matter, when rarified or dispers'd, be either fill'd as to sense, or, at least, made (as they speak) the sphere of its activity.

To manifest this Truth, and thereby as well confirm the foregoing Chapter, as make out what is design'd in this, I shall endeavour to shew, and help your imagination to conceive, how great a space may be impregnated with the Effluxions of a Body, sometimes without any sensible, and sometimes without any considerable decrement

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crement in bulk or weight of the Body that affords them. And in order to this, though I shall not pretend to determine precisely how little the substances, I am to instance in, would waste upon the Ballance, because you will very easily see they are not that way to be examin'd; yet I presume, you will as easily grant, that the decrement of weight would be but inconsiderable, since of such light substances the loss even of bulk is so; which last clause I shall now attempt to make good, by setting down some Observations, partly borrow'd from the writings of approv'd Physicians, and partly that my friends and I have made about the durable Evaporation of such small particles of the Efflu- xions of Animals, as are actually not to be discern'd by the Eye to have any of those things sticking to them, which are so very long in flying suc- cessively away.

'Tis wont to be somewhat surpris-
ing to men of Letters, when they
first go a hawking with good Spa-
niels,

hounds, to observe, with how great sagacity those dogs will take notice of, and distinguish by the scent, the places where Partridges, Quails, &c. have lately been. But I have much more wonder'd at the quick scent of an excellent Setting-dog, who by his way of ranging the fields, and his other motions, especially of his Head, would not only intimate to us the kinds of game, whose scent he chanc'd to light on, but would discover to us where Partridges had been (though perhaps without staying in that place) several hours before, and assist us to guess how long they had been gone before we came.

I have had strange answers given me in *Ireland*, by those who make gain if not an intire livelihood by killing of Wolves in that Countrey, where they are paid so much for every head they bring in) about the sagacity of that peculiar race of dogs they imploy in hunting them; but not trusting much to those Relators, I shall add, that a very sober and discreet

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discreet Gentleman of my acquaintance, who has often occasion to imploy Blood-hounds, assures me, that if a man have but pass'd over a field, the scent will lye (as they speak) so as to be perceptible enough to a good dog of that sort for several hours after. And an ingenious Hunter assures me, that he has observ'd, that the scent of a flying and heated Deer will sometimes continue upon the ground from one day to the next following.

And now we may consider these three things; *First*, That the substance left upon the grass or ground by the transient-tread of a Partridge, Hare, or other *animal*; that does but pass along his way, does probably communicate to the grass or ground but some of those Effluxions; that transpire out of his feet, which being small enough to escape the discernment of the Eye, may probably not amount to one grain in weight, or perhaps not to the tenth part of it. *Next*, That the parts of fluid Bodies,

as

such, are perpetually in motion, and so are the invisible particles that swim in them, as may appear by the dissolution of Salt or Sugar in water, and the wandering of aqueous Vapours through the Air, even when the Eye perceives them not. And *thirdly*, That though the Atmosphere of one of these small parcels of the exhaling matter we are speaking of, may oftentimes be exceeding vast in comparison of the emittent Body, as may be guess'd by the distance, at which some Setters, or Blood-hounds, will find the scent of a Partridge, or Deer; yet in places expos'd to the free air or wind, it is very likely that these steams are assiduously carried away from their fountain, to maintain the fore-mention'd Atmosphere for six, eight, or more hours, that is, as long as the scent has been observ'd to lye, there will be requisite a continual recruit of steams succeeding one another: and that so very small a portion of matter as that which we were say-
ing

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ing the *fomes* of these steams may be judg'd to be, being sensibly to impregnate an Atmosphere incomparably greater than it self, and supply it with almost continual recruits we cannot but think, that the steams it parts with, must be of an extreme and scarce conceivable minuteness.

And we may further consider that the substances, which emit these steams, being such as newly belong to Animals, and were, for the most part, transpir'd through the pores of their feet, must be in likelihood far more evaporable and dissipable kind of Bodies than Minerals or adult Vegetables, such as Gunpowder is made of; so that if the grains of Gunpowder emit Effluvia capable of being by some Animals perceived at a distance by their smell one may probably suppose, that the small grains of this powder may hold out very many times longer to supply an Atmosphere with odorable steams, than the Corpuscles left on the

the ground by transient Animals.

Now though it be generally agree'd on, that very few Birds have any thing near so quick a sense of smelling as Setting-dogs or Blood-hounds, yet that the odour of Gunpowder, especially when assisted by the steams of the *Caput mortuum* of Powder formerly fir'd in the same Gun, may by Fowls be smelt at a notable distance, particularly when the wind blew from me towards them, I often perswaded my self I observ'd, especially as to Crows, when I went a shooting; and was confirm'd in that opinion, both by the common Tradition, and by sever and ingenious persons much exercis'd in the killing of Wild-fowl, and of some fourfooted Beasts.

I had forgotten to take notice of one Observation of the experienc'd *Julius Palmarius*: Whence we may learn, that Beasts may leave upon the Vegetables, that have touch'd their bodies for any time, such Coruscules; as, though unheeded by other,

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other Animals, may, when eaten by them, produce in them such diseases as the infected Animals had. For this Author writes in his useful *Traët de morbis Contagiosis*, that he observ'd Horses, Beeves, Sheep and other Animals, to run mad upon the eating of some of the straw on which some mad Swine had layn.

And now to resume and prosecute our former discourse, you may take notice, that the *Effluvia*, mention'd to have been smelt by Animals, are, though invisible, yet big enough to be the objects of sense; so that 'tis not improbable, that, among the steams that no sense can immediately perceive, there should be some far more subtil than these, and consequently capable of furnishing an Atmosphere much longer, without quite exhausting the effluviating matter that afforded them.

* *Forestus*, an useful Author, recites an Example of Pestilential contagion long preserv'd in a Cobweb.

* *Lib. 6. Observ. 22.*

Alexan

Alexander Benedictus writes also, that at *Venice* a Flock-bed did for many years harbour a pestiferous malignity to that degree, that when afterwards it came to be beaten, it presently infected the by-standers with the Plague.

And the Learned * *Sennertus* himself relates, that in the year 1542. there did in the City of *Uratistavia* (vulgarly *Breslaw*;) where he afterwards practised Physick; dye of the Plague, in less than six Months, little less than six thousand men, and that from that time the Pestilential Contagion was kept folded up in a linen cloth about fourteen years, and at the end of that time being display'd in another City, it began Plague there, which infected also the neighbouring Towns and other places.

* *Trincavella* makes mention of yet lastinger Contagion, (which occasion'd the death of ten thousand

* *Lib. 4. de Feb. cap 3.*

* *Libr. 3. Con. 17.*

E persons)

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persons) that lay lurking in certain Ropes, with which at *Justinopolis* those that dy'd of the Plague had been let down into their Graves.

But, though none of these Relations should to some Criticks appear scarce credible, it may be objected, that all these things, wherein this Contagion resided, were kept close shut up, or at least were not expos'd to the Air. Wherefore having only intimated, that the exception, which I think is not irrational, would, though never so true, but *lessen* the wonder of these strange Relations, without rendering them *unfit* for our present purpose, I shall add; that though 'tis the opinion of divers Learned Physicians, that the matter harboring Contagion cannot last above Twenty or a few more days, if the Body it adheres to be expos'd to the free air and the wind, and though I am not forward to deny, that their judgement may hold in ordinary cases; yet I must not deny neither, that a Contagion may sometimes

times happen to be much more tenacious and obstinate : Of which I shall give but that one , almost recent instance, observ'd by the Learned * *Dimmerbrook* in his own Apothecary , who having but remov'd with his foot , from one side to the other of a little Arbour (in his Garden) some straw, that had layn under the Pallet , on which near eight Months before a Bed had layn , wherein a Servant of the Apothecaries , that recover'd , had been sick of the Plague ; the infectious steams presently invaded the lower part of his leg , and produc'd a pungent pain and blister , which turn'd to a pestilential Carbuncle, that could scarce be cur'd in a Fortnight after , though during that time the Patient were neither feaverish , nor, as to the rest of his Body , ill at ease. This memorable instance , together with some others of the like kind , that our Author observed in the same City (of *Nimwegen*) obtain'd , not to say,

* *Lib. 4. de Peste.*

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extorted, even from him, this Confession ; which I add , because it contains some considerable, and not yet mentioned Circumstances of the recited case : *Hoc exemplo Medicorum Doctrina de Contagio in fomite latente satis confirmatur. Mirum tamen est, hoc Contagium tanto tempore in prædicto stramine potuisse subsistere, utpote quod tota hyeme ventis & pluviis, (he adds in another place) nivibus & frigori, expositum fuisset.*

And now I will shut up this Chapter with an instance, that some will think, perhaps, no less strange than any of the rest, which is, that though they that are skilful in the perfuming of Gloves, are wont to imbue them with but an inconsiderable quantity of odoriferous matter, yet I have by me a pair of *Spanish* Gloves, which I had by the favour of your fair and virtuous Sister (*F.*) that were so skilfully perfum'd, that partly by her, partly by those, that presented them her as a Rarity, and partly by me, who have kept them
several

Several Years, they have been kept about eight or nine and twenty years, not thirty, and they are so well scented, that they may, for ought I know, continue fragrant divers years longer. Which instance, if you please to reflect upon, and consider, that such Gloves cannot have been carried from one place to another, or so much as uncover'd (as they must often have been) in the free Air, without diffusing from themselves a fragrant Atmosphere, we cannot but conclude those odorous Steams to be unimaginably subtle, that could for so long a time issue out in such swarms, from a little perfum'd matter lodged in the pores of a Glove, and yet leave it wholly stock'd with particles of the same nature; though, (especially by reason of some removes, in which I took not the Gloves along with me,) I forgot ever since I had them, to keep them so much as shut up in a Box.

Of the
GREAT EFFICACY
OF
EFFLUVIALS:

BY
The Honorable ROBERT BOYLE.

OF
GREAT EFFICACY
OF
TETTERLIVUM 3.

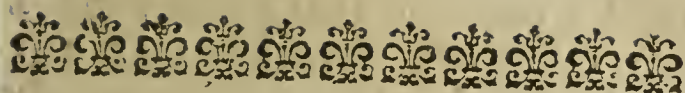
BY
THE HONORABLE ROBERT BOYLE.

OF THE
Determinate Nature
OF
EFFLUVIUMS.

A

OF

THE
HISTORICAL RECORDS
OF
SMITHVILLE



OF THE
DETERMINATE NATURE
OF
EFFLUVIUMS.

CHAP. I.

THE Effluvia of Bodies;
Pyrophilus, being for the most
part invisible, have been wont
to be so little consider'd by vulgar
Philosophers, that scarce vouchsafing
to take notice of their Existence, 'tis
no wonder that men have not been
solicitous to discover their distinct
Natures and Differen-
ces. Only * *Aristotle*, * *Lib. 1. Meteor.*
and (upon his account) *cap. 3, & 4.*
the Schools, have been pleased to
A think,

think, that the two grand parts of our Globe do sometimes emit two kinds of Exhalations or Steams; the *Earthy* part affording those that are hot and dry, which they name *Fumes*, and very often, simply, *Exhalations*; and the *Aqueous* part, others that are (not as many of his Disciples mistake him to have taught, Cold and Moist,

but) Hot and Moist*, which they usually call *Vapours*, to discriminate them from the

Fumes(or *Exhalations*,) though otherwise, in common acceptation, those Appellations are very frequently confounded.

But, though the *Aristotelians* have thus perfunctorily handled this Subject, it would not become Corpuscularian Philosophers, who attribute so much as they do to the Insensible Particles of Matter, to acquiesce in so slight and jejune an account of the Emanations of Bodies. And since we have already shewn, that besides the greater and more simple Masses

of

* Cap. 3. Ἐστὶ γὰρ ἀτμίδες καὶ ὑγροὶ καὶ θερμαίνοντες.

of Terrestrial and Aqueous matter newly mention'd, there are very many mixt Bodies, that emit Effluvia, which make, as it were, little Atmospheres about divers of them, it will be congruous to our Doctrine and Design, to add in this place, That besides the slight and obvious differences, taken notice of by *Aristotle*, the Steams of Bodies may be almost as various as the Bodies themselves that emit them; and that therefore we ought not to look upon them barely under the general and confused notion of Smoak or Vapours, but may probably conceive them to have their distinct and determinate Natures, oftentimes (though not always) suitable to that of the Bodies from whence they proceed.

And indeed the newly mentioned Division of the *Schools* gives us so slight an account of the Emanations of Bodies, that, methinks, it looks like such another, as if one should divide Animals into those that are *Horned*, and those that have *Two Feet*:

For, besides that the Distinction is taken from a Difference that is not the considerablest, there are divers Animals (as many four-footed Beasts and Fishes) that are not comprised in it; and each member of the Division comprehends I know not how many distinct sorts of Animals, whose differences from one another are many times more considerable, than those that constitute the two supreme *Genus's*, the one having Bulls and Goats, and Rhinoceros's, and Deer, and Elks, and certain Sea-Monsters whose Horns I have seen; and the other *Genus* comprising also a greater Variety, namely, a great part of Four-footed Beasts, and, besides Men, all the Birds (for ought we know) whether of Land or Water. And as it would give us but a very slender Information of the Nature of an Elk or an Unicorn, to know that 'tis an *Horned* Beast; or of the Nature of a Man, an Eagle, or a Nightingale, to be told, that 'tis an *Horn-less* Beast; so it will but very little instruct a man
in

Nature of Effluvia.

7

in the Nature of the Steams of *Quick-
silver* or of *Opium*, to be told, that
they are Vapours Hot (or rather
Cold) and Moist; or of the Steams
of *Amber* or *Cantharides*, or *Cinnamon*,
or *Tobacco*, to be told, that they are
Hot and Dry. For, besides that
there may be Effluvia, which, even
by their Elementary Qualities, are
not of either of these two supreme
genus's, (for they may be Cold and
Dry, or Cold and Moist,) these Qua-
ties are often far from being the
loblest, and consequently those that
deserve to be most consider'd in the
Effluvia of this, or that, Body;
we shall by and by have occasion
to manifest.

CHAP. II.

And here it may not be im-
proper to mention an Experiment;
that, I remember, I divers years since

A 4

em-

employed to illustrate the Subject of our present Discourse.

I consider'd then, that Fluid Bodies may be of very unequal density and gravity, as is evident in Quick-silver, Water, and pure Spirit of Wine, which, notwithstanding their great difference in specifick gravity, may yet agree in the conditions requisite to Fluid Bodies. Therefore presuming, that by what I could make appear visible in one, what happens analogically in the other, may be particularly illustrated, I took some Ounces of Roch-allom, and as much of fine Salt-peter. I took *some Ounces* of each, because, if the quantity of the ingredients be too small, the concoagulated grains will be so too, and the success will not be so conspicuous. These being dissolved together in fair Water, the filtrated solution was set to evaporate in an open-mouthed Glass, and being then left to shoot in a cool place, there were fastned to the sides and other parts of the Glass several small Crystals, some Octoedrical, which

which is the figure proper to Roch-
allom, and others of the Prismatical
shape of pure Salt-peter; besides some
other Saline concretions, whose be-
ing distinctly of neither of these two
shapes, argued them to be concoagu-
ations of both the Salts. And this
we did by using such a degree of Ce-
erity in Evaporating the liquor, as
was proper for such an effect. For,
by another degree, which is to be
employ'd when one would recover
the Salts more distinctly and mani-
festly, the matter may (as I found
by tryal) be so ordered, that the alu-
minous Salt may, for the most part,
be first coagulated by it self, and then
from the remaining liquor curiously
shap'd Crystals of Nitre may be co-
piously obtained.

Tryals like this we also made with
other Salts, and particularly with Sea-
salt, and with Allom and Vitriol;
the *Phænomena* of which you may
meet with in their due places. For
the recited Experiment may, I hope,
alone serve to assist the imagination
to

10 Of the Determinate

to conceive, how the Particles of Bodies may swim to and fro in a Fluid (which the Air is,) and though they be little enough to be invisible, many of them retain their distinct and determinate natures, and their aptness to cohere upon occasion; and others may, by their various occurrences and coalitions, unite into lesser Corpuscles or greater Bodies differing from the more simple Particles, that composed them, and yet not of indeterminate though compounded Figures.

CHAP. III.

THESE things being premis'd, we may now proceed to the particular Instances of the Determinate Nature of Effluvia, and these we may not inconveniently reduce to the three following Heads, to each of which we shall assign a distinct Chapter; the first of these I shall briefly

Nature of Effluvioms. 11

chiefly treat of in this third Chapter, and treat somewhat more largely of the others in the two following.

In the first place then, That the Effluvioms of many Bodies retain determinate Nature oftentimes in an invisible smallness, and oftener in such a size as makes them little enough to fly or swim in the Air; may appear by this, that these *Effluvia* being Condensation or otherwise reunited, they appear to be of the same Nature with the Body that emitted them. Thus in moist weather, the vapours of Water, that wander invisibly through the Air, meeting with Marble-Walls or Pavements, or other Bodies, by their Coldness and other Qualifications, fit to condense and retain them, appear again in the form of Drops of Water; and the same Vapours return to the visible form of Water, when they fall out of the Air in Dews, or Rains.

Quicksilver it self, if it be made ascend in distillation with a convenient degree of Fire, will almost all be

be found again in the Receiver in the form of running Mercury. Which strange and piercing Fluid, is in some cases so disposed to be strip'd of its Disguises, and re-appear in its own form that divers Artificers, and especially Gilders, have found, to their cost that the fumes of it need not be, as in Distillation, included in close Vessels to return to their pristine nature. Mercury having been several times found in the Heads and other parts of such People, who have in tract of time been killed by it, and some times made to discover it self during the Lives of those that dealt so much in it; of which I elsewhere give some Instances. Wherefore I shall only observe at present, that 'tis common Practice, both among Gilders, and some Chymists, that when they have occasion to make an Amalgam, or force away the Mercury from one by the fire, they keep Gold in their Mouthes, which by the Mercurial fumes, that wander through the Air, will now and then

y that time 'tis taken out of their mouths, be turned white almost, as if it had been silver'd over.

A mass of purified Brimstone being sublimed; the ascending fumes will condense into what the Chymists call *Alores Sulphuris*, which is true Sulphur of the same nature with that, formerly exposed to sublimation; and may readily by melting be reduced into such another mass.

And to give you another like Example of dry Bodies; I tryed, that by subliming good Camphire in close vessels, it would all, as to sense, be raised into the upper vessel, or part of the Subliming-glass in the form of dry Camphire as it was before.

Nay though a Body be not by Nature, but Art compounded of such differing Bodies as a Metal and another Mineral, and two or three Salts; yet, if upon Purification of the mixture from its grosser parts, the remaining and finer parts be minute enough and fitly shap'd, the whole liquor will ascend; and yet in the Receiver

Receiver altogether recover its pristine form of a transparent Fluid composed of differing Saline and Mineral parts. This is evident in the Distillation of what Chymists call *Butter*, or *Oyl* of Antimony, very well rectified. For, this Liquor will pass into the Receiver diaphanous and fluid, though, besides the Particles of the Sublimate, (which is it self a factitious compounded Body) it abounds with Antimonial Corpuscles, carried over and kept invisible by the corroding Salts; whatever *Angelus Sala*, and those Chymists that follow him, have affirm'd to the contrary; as might be easily here proved, if this were a fit place to do it in.

I found by inquiring of an Ingenious person, that had an interest in a Tin-Mine, that I was not deceived in guessing, that Tin it self, though a Metal whose Ore is of a very difficult fusion, and which I have by it self kept long upon the Cupel without finding it to fly away, would yet retain its Metalline nature in the form of
of

of fumes or flowers. For this experienc'd Gentleman answer'd me, that divers times they would take great store of a whitish Sublimate from the upper part of the Furnaces or Chimnies, where they brought their Ore to fusion, or wrought further upon it; and that this Sublimate, though perhaps elevated to the height of an ordinary Man, would, when melted down, afford at once many Pounds of very good Tin. On which occasion I shall add, that I have my self more than once rais'd this Metal in the form of white Corpuscles by the help of an Additament, that did scarce weigh half so much as it.

CHAP. IV.

THE second way, by which we may discover the Determinate Nature of *Effluvia*, is, by the difference that may sometimes be observ'd in their Sensible Qualities. For, these

these Effluviūms that are endow'd with them, proceed from the same sort of Bodies, and yet those afforded by one kind of Bodies being in many cases manifestly differing from those that fly off from another, this evident disparity in their Exhalations argues their retaining distinct natures, according to those of the respective Bodies whence they proceed.

I will not now stay to examine, whether in the Steams, that are made visibly to ascend from the Terrestrial Globe by those grand Agents and usual raisers of them, the Sun, and the agitation of the Air; the Eye can manifestly distinguish the diversity of colours: But in some productions of Art such different colours may be discovered in the Exhalations, even without the application of any external heat to raise them. For, when Spirit of Nitre, *for example*, has been well rectified, I have often observ'd, that even in the cold the fumes would play in the unfill'd part of the stop'd Vials it was kept in, and appear in it

Nature of Effluvioms. 17

of a reddish colour, and, if those vessels were open'd, the same fumes would copiously ascend into the Air, in the form of a reddish or orange-woolny Smoak. Spirit or Oyl of Salt-peter, if it be very well dephlegm'd, though it will scarce in the cold violently ascend in the empty part of a Vial, whilst it is kept well stop'd; yet, if the free Air be allow'd access to it, it will, in case it be sufficiently rarified, fly up in the form of a whitish fume. But this is inconsiderable in comparison of what happens in a volatile Tincture of Sulphur; I have elsewhere taught you to make it with Quick-lime. For, not only upon a light occasion the vacant part of the Vial will be fill'd with white fumes, though the Glass be well stop'd; but upon the opening the Vial these fumes will copiously pass out at the Neck, and ascend into the Air in the form of a Smoak, more white than perhaps you ever saw any. And both this and that of the Spirit of Salt-peter, by their operation, as well as smell,

B disclose

disclose what they are; the latter being of a Nitrous nature, (as is confess'd.) and the former, of a Sulphureous: In so much that having for curiosities sake in a fitly shap'd Glass caught a competent quantity of the ascending white fumes, I found them to have conven'd into Bodies transparent and Geometrically figur'd, where in 'twas easie to discover by their sensible qualities, that there were store of Sulphureous particles mixt with the Saline ones. That the liquors of Vegetables, distill'd in *Balneo* or in Water, are not wont to retain any thing of the colour of the Bodies that afforded them, is a thing easie to be observ'd in Distillations made without Retorts or the violence of the Fire. But it may be worth while to make tryal, whether the Essential Oyl of Wormwood ascend colour'd like the Plant, whence 'tis first drawn over with Water in the Limbeck, or rectified in *Balneo*. For, I forgot to take notice of it, when upon some particularities, I observ'd in that Plant

my

my curiosity led me to find, that not only in the first distillation in a Copper Limbec, tinn'd on the inside, the Oyl came over green, but by a rectification purposely made in a Glass-vessel, the purified liquor was not deriv'd of that colour.

The mention of these Essential Oyls, as Chymists call those that are drawn in Limbecs, leads me to tell you, that, though these liquors be but *Effluvia* of the Vegetables they are distill'd from, condens'd again in the Receiver into liquors; yet, as subtile as they are, many of them retain the genuine taste of the Bodies, whence the heat elevated them; as you will easily find, if you will tast a few drops of the Essential Oyl of Cinamon, *for example*, or of Wormwood dissolv'd by the intervention of Sugar or Spirit of Wine in a convenient quantity of Water, Wine, or Beer. For, by this means you have the natural taste of this Spice or Herb. And Wormwood is a Plant, whose *Effluvia* do so retain the nature of the Body that parts with

B 2 them,

them, that I must not forbear to alledge here an Observation of mine, that may shew you, that 'tis possible, though not usual, that even without the help of the Fire the expirations of a Body may communicate its tast. For, among other things, that I had occasion to observe about some quantity of Wormwood laid up together, I remember, I took notice, and made others do the like, that coming into a room, where 'twas kept, not only the organs of smelling were powerfully wrought upon by the Corpuscles that swarm'd in the Air, but also the Mouth was sensibly affected with a bitter tast. Perhaps you will scarce think it worth while, that after this instance I should add, that I found the expirations of *Amber*, kept a while in pure Spirit of Wine, tast upon the tongue like Amber it self, when I chew'd it between my teeth. But I choose to mention this instance, because it will connect those lately mention'd with another sort, very pertinent to our present purpose. For,
the

the expirations that I have obtain'd from Amber, both with pure Spirit of Wine, and a more piercing *Mentruum*, did manifestly retain in both those liquors a peculiar smell; with which I found it to affect the Nostrils, when, for tryals sake, I excited the Electrical faculty of Amber by rubbing. And as for Odours, 'tis plain, that the Essential Oyls of Chymists, well drawn, do many of them retain the peculiar and genuine scent of the Spices or Herbs that afforded them. And that these Odours do really consist of, or reside in certain invisible Corpuscles that fly off from the visible Bodies, that are said to be endow'd with such Smells, I have elsewhere prov'd at large; and it may sufficiently appear from their sticking to divers of the Bodies they meet with, and their lasting adhesion to them.

Other Examples may be given of the settled difference of Effluvia directly perceivable by Humane Organs of Sense, as dull as they are;

which last expression I add, because I scarce doubt, but that, if our Sensories were sufficiently subtile and tender, they might immediately perceive in the size, shape, motion, and perhaps colour too of some now invisible Effluviiums, as distinguishable differences, as our naked Eyes in their present constitution see, between the differing sorts of Birds, by their appearances, and their manner of flying in the Air, as Hawks, and Partridges, and Sparrows, and Swallows. To make this probable I will not urge, that in fine white Sand, whose grains by the unassisted Eye are not wont to be distinguished by any sensible Quality, I have often observ'd in an excellent Microscope, a notable disparity as to bulk, figure, and sometimes as to colour: And that in small Cheese-mites, which the naked Eye can very scarcely discern, so far is it from discovering any difference between them, one may (as was noted in the last essay) plainly see, besides an obvious difference in point of bigness, many

many particular parts, on whose accounts the structure of those *moving points* may difference them from each other. And I have sometimes seen a very evident disparity even in point of shape between the very Eggs of these *living Atoms*, (as a Poet would perhaps stile them.) But these kinds of proofs (as I was saying) I shall forbear to insist on, that I may proceed to countenance my conjecture by the effects of the Effluvia, that are properly so call'd, upon Animals.

And first, though the Touch be reckon'd one of the most dull of the five Senses, and be reputed to be far less quick in Men than in divers other Animals; yet the gross Organs of that, may, in Men themselves, even by accident, be so dispos'd, as to be susceptible of impressions from *Effluvia*: Of this in another Paper I give some Instances. And I know not whether divers of the Presages of Weather to be observ'd in some Animals, and the Aches and other pains, that, in many crazy and wounded

men, are wont to fore-run great changes of Weather, do not often (for I do not say alwayes) proceed (at least in part) from invisible and yet incongruous Effluxions, which, either from the subterranean parts, or from some Bodies above ground, do copiously impregnate the Air. And on this occasion it will not be impertinent to mention here what an experienc'd Physician being (if I much misremember not) the Learned *Dimmerbrook*, relates concerning himself, who having been infected with the Plague by a Patient that lay very ill of it, though by Gods blessing, which he particularly acknowledges, upon a slight but seasonable Remedy, he was very quickly cured, and that without the breaking of any Tumor; yet it left such a change in some parts of his Body, that he subjoyns this memorable passage; *Ab illo periculo ad contagiosos mihi appropinquantibus in emunctoriis successit dolor, vix fallax Pestis indicium.*

Two or three other Observations
of

of the like nature you meet with in another of my Papers *.

And I shall now add, * About Cosmi-
cal Suspitions.
that I know an ingeni-

ous Gentlewoman (Wife to a famous Physician) who was of a very curious and delicate complexion, that has several times assur'd me, that she can very readily discover, whether a person, that comes to visit her in Winter, came from some place where there is any considerable quantity of Snow; and this she does, (as she tells me) not by feeling any unusual cold (for if the ground be frozen but not cover'd with Snow, the Effect succeeds not,) but from some peculiar impression, which she thinks, she receives by the organs of Smelling. I might add, that I know also (as I may have formerly told you) a very ingenious Physician, who falling into an odd kind of Fever, had his sense of Hearing thereby made so very nice and tender, that he very plainly heard soft whispers, that were made at a considerable distance off, and which were
not

not in the least perceiv'd by the healthy by-standers, nor would have been by him before his sickness. Which (sickness) I mention as the thing, that gave his organs of Hearing this preternatural quickness, because when the Feaver had quite left him, he was able to hear but at the rate of other men. And I might tell you too, that I know a Gentleman of eminent parts and note, who, during a distemper he had in his Eyes, had his organs of Sight brought to be so tender, that both his friends and himself also have assur'd me, that when he wak'd in the Night he could for a while plainly see and distinguish Colours, as well as other objects, discernable by the Eye, as was more than once try'd, by pinning Ribbands or the like Bodies of several colours, to the inside of his Curtains in the dark. For if he were awaken'd in the Night, he would be able to tell his bed-fellow, where those Bodies were plac'd, and what colour each of them was of.

I have

I have mention'd these Instances
only to shew you, that if our Senso-
es were more delicate and quick,
they would be sufficiently affected by
jects, that, as they are generally
nstituted, make no impressions at
l upon them. For otherwise I know,
at the *Species* (as they call them) both
Sounds and Colours, are not held
y many of the Moderns, (from whom
that I dissent not,) to be so much
orporeal Effluxions, trajected through
e *medium*, as peculiar kinds of Lo-
l Motion convey'd by it. There-
re I shall now confirm the conje-
ure I would countenance by the dis-
imination made by the organs of
her Animals of such *Effluvia* as to
men are not only invisible but in-
nsible. And therefore partly to
ngthen what I deliver'd, and
rtly to confirm what I am now dis-
oursing of, it will not be imperti-
ent to subjoyn two or three Rela-
ons, that I had from persons of very
ood credit, whom I thought likely
o make me no unsatisfactory returns
to

to my Questions about things they were very well vers'd in.

A person of Quality, to whom I am near allied, related to me, that to make a tryal, whether a young Blood-hound was well instructed, (or as the Huntsmen call it, *made*) he caus'd one of his Servants, who had not kill'd, or so much as touch'd any of his Deer, to walk to a Countrey-town, four Mile off, and then to a Market-town three Miles distant from thence; which done, this Nobleman did, a competent while after, put the Blood-hound upon the scent of the man, and caus'd him to be follow'd by a Servant or two, the Master himself thinking it also fit to go after them to see the event; which was that the Dog, without ever seeing the Man he was to pursue, follow'd him by the scent to the above-mentioned places, notwithstanding the multitude of Market-people that went along in the same way, and of Travellers that had occasion to cross it. And when the Blood-hound came to the chief

Chief Market-town, he pass'd through the streets, without taking notice of any of the people there, and left not till he had gone to the House, where the Man, he sought, rested himself, and found him in an upper Room to the wonder of those that follow'd him. The particulars of this Narrative the Nobleman's Wife, a person of great eracity, that happen'd to be with him when the tryal was made, confirm'd to me.

Enquiring of a studious person, that was Keeper of a Red-deer-park and vers'd in making Blood-hounds, how long time, after a Man or Deer had pass'd by a grassy place, one of those Dogs would be able to follow him by the scent? He told me, that it would be six or seven Hours: Whereupon an ingenious Gentleman, that chanc'd to be present, and liv'd near that Park, assur'd us both, that he had old Dogs of so good a scent, that if a Buck had the day before pass'd in a Wood, they will, when they come where the scent lies, though

though at such a distance of time after, presently find the scent and run directly to that part of the Wood where the Buck is. He also told me that though an old Blood-hound will not so easily fix on the scent of a single Deer, that presently hides himself from a whole herd; yet if the Deer be chas'd a little till he be heated, the Dog will go nigh to single him out though the whole herd also be chas'd. The above-nam'd Gentleman also affirm'd, that he could easily distinguish whether his Hounds were in chase of a Hare or a Fox by their way of running, and their holding up the Nose higher than ordinary when they pursue a Fox; whose scent is most strong. These Relations will not be judg'd incredible by him that reflects on some of the Instances that have already (in the foregoing Essay) been given of the strange subtilty of *Fluvia*: To which I shall now add that I remember, that to try whether I could in some measure make Animals imitate Nature, I prepared a Book

of a vegetable substance, which, though it were actually cold, and both to the Eye and Touch dry, did for a while emit such determinate and piercing, though invisible, Exhalations, that having for Tryals sake applied to it a clear Metalline Plate (and that of none of the very softest kind neither) for about one Minute of an Hour, I found, that, though there had been no immediate contact between them, I having purspofely interposed a piece of Paper to hinder it; yet there was imprinted on the surface of the Plate a conspicuous stain of that peculiar colour, that the Body, with whose Steams I had imbued the vegetable substance, was fitted to give a Plate of that mix'd Metal. And though it be true, that in some circumstances, the lately mention'd instances about Blood-hounds have considerable advantage of this I have now recited; yet that advantage is much lessen'd, not to say countervail'd, by some circumstances of our Experiment. For, not to repeat, that

that the emittent Body was firm and cold, the Effect produced by the Effluvium that guided the Setting-dog, was wrought upon the Senfory of a living and warm Animal; and such an one, whose organs of Smelling are of an extraordinary tender Constitution above those of Men and other Animals, and probably the Impression was but transient; whereas in our case the invisible Steams of the vegetable substance wrought upon a Body which was of so strong and inorganical a Texture as a (compounded) Metal, though it were fenc'd by being lapt up in Paper, notwithstanding which these Steams invaded it in such numbers, and so notably, as to make their Operation on it manifest to the Eye, and considerably permanent too; since coming to look upon the Plate after the third day, I found the induced Colour yet conspicuous, and not like suddenly to vanish.

Hitherto in this *Chapter* I have argued from the constant and settled
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difference of the sensible Qualities of Effluvia, that they do not always lose their distinct natures, when they seem to have lost themselves by vanishing into Air. But before I discuss this Subject, I must consider an Objection, which I know may be made against the Opinion we have been countenancing. For it may be ledg'd, that there may be many cases, wherein the Effluvia of Bones are, in their passage through the Air, sensibly alter'd, or do affect the organs of sense otherwise than each and of them apart would do: Nor is this difficulty altogether irrational. For it seems consonant enough to Experience, that some such cases should be admitted; and therefore in the foregoing Discourse I have, where I thought it necessary, forbore to express myself in such general and absolute terms, as otherwise I might have done. But, as for such cases as I have insisted upon, and many more, I shall now represent, that the objected alterations need not hinder, but that

Effluvia at their first parting from the Bodies, whence they take wing (if I may so speak,) may retain as much of the nature of those Bodies, as we have ascribed to them; since the subsequent change may very probably be deduc'd from the combinations or coalitions of divers Steams associating themselves in the Air, and acting upon the Sensory, either altogether and conjointly, or at least so near it, that the Sense cannot perceive their Operations as distinct. This I shall elucidate, but not pretend to prove, by what happens in Sounds and Tasts. For if, by way of instance, in a Musical instrument two strings tun'd to an eighth, be touch'd together, they will strike the Ear with a sound, that will be judg'd one, as well as pleasing, though each of the trembling strings make a distinct noise, and the one vibrates as fast again as the other. And if, into Oyl of Tartar *per Deliquium*, you drop a due proportion of Spirit of Nitre and exhale the superfluous moisture

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the Acid and Alcalizate Corpuscles, that were so small as to swim invisibly in those liquors, will convene into Nitrous Concretions; whose taste will be compounded of, but very differing from, both the tastes of the Acid and Tartareous Particles; which Particles may yet, for the most part, by a skilful Distillation, be divorc'd again. And so, if to a strong solution of Pot-ashes or Salt of Tartar you put as much in weight of *Sal armoniack*, as there is of either of those last Salts contain'd in the liquor; you may, besides a subtil Urinous spirit that will easily come over in the distillation, obtain a dry *Caput mortuum*, which is almost totally a compounded Salt, differing enough from either of the ingredients; especially the Alcalizate, as well in Taste as in some other Qualities: This Salt (free'd from its *faces*) being that Distick Salt, I several years ago gave quantities of, to some Chymists and Physicians, from the most of whom I received great thanks, accompanied

with the (more acceptable) accounts of the very happy success they had employed it with, though usually but in a small Dose, as from six, eight or ten Grains to a Scruple. But this being mentioned only upon the by, I shall proceed to tell you, that, since I intimated to you already, that I would mention Examples of *Sound* and *Tastes* only to illustrate what I had been delivering; I shall now add some Instances by way of Proof, of the Coalition and resulting change of Steams in the Air. 'Tis easily observable in some Nose-gays, where the differing Flowers happen to be conveniently mix'd, that in the smell afforded by it, at a due distance, the Odours of the particular Flowers are not perceiv'd, but the Organ is affected by their joynt-action, which makes on it a confused but delightful impression. And so, when in a Bag of Pomander, or a perfum'd Skirt Musk, and Amber, and Civet; and other sweets are skilfully mix'd, the coalition of the distinct *Effluvia* of the
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Ingredients, that associate themselves in their passage through the Air, produce in the Sensory one grateful perfume, resulting from all those Odours. But if you take Spirit of fermented Urine and Spirit of Wine, both of them Phelgmatick, and mix them together, they will incorporate like Wine and Water, or any other such liquors, without affording any dry concretions. But if you expose them in a convenient Vessel but to the mild heat of a Bath or Lamp, the ascending Particles will associate themselves, and adhere to the upper part of the Glass in the form of a white but tender Sublimate, consisting both of Urinous and Vinous Spirits, associated into a mixture, which differs from either of the liquors, not only in Consistence, Taste and Smell, but in some considerable Operations performable by this odd mixture; which, this is not the place, to take further notice of. And if Spirit of Salt and Spirit of Nitre be, by Distillation, elevated in the form of Fumes,

38 Of the Determinate

so order'd as to convene into one liquor in the Receiver, this liquor will readily dissolve crude Gold, though neither the Spirit of Nitre alone, nor that of Salt would do so.

And that you may have an ocular proof of the Possibility of the distinctness and subsequent Commixture of Steams in the Air; I shall now add an Experiment, which I long since devis'd for that purpose, and which I soon after shew'd to many curious persons, most of whom appear'd somewhat surpriz'd at it. The Experiment was; that I took two small Vials, the one fill'd with Spirit of Salt, but not very strong, the other with Spirit of fermented Urine or of *Sal Armoniack* very well rectified: These Vials being plac'd at some distance, and not being stopp'd, each liquor afforded its own smell at a pretty distance, by the Steams emitted into the Air, but yet the Steams were invisible. But when these Vials, (which should be of the same size) came to be approach'd

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very near to each other, though not so, as to touch; as when the two liquors are put together in the form of liquors, they will notably act upon one another; so their respective Effluvioms meeting in the Air, would, answerably to the littleness of their bulk, do the like, and, by their mutual occurrsions, become manifestly visible, and appear moving in the Air like a little portion of Smoak or of a Mist, which would quickly cease, if either of the Vials were remov'd half a Foot or a Foot from the other. And I remember, that, to add to the oddness of the *Phenomenon*, I sometimes made a drop of the Spirit of Salt hang at the bottom of a little stick of Glass or some other convenient Body, and held this drop thus suspended in the Orifice of a Vial that had Spirit of *Sal Armoniack* in it, and was furnish'd with a somewhat long neck; for by this means it happen'd, as I expected, that the ascending Urinous Particles, though invisible before, invading plentifully the

Acid ones of the drop, produced a notable Smoak, which, if the drop were held a little above the neck of the Glass, would most commonly fly upwards to the height of a Foot or half a Yard: But if the drop were held somewhat deep within the Cavity of the neck, a good part of the produced Smoak would oftentimes fall into the Cavity of the Vial, which was left in great part empty, sometimes in the form of drops, but usually in the form of a slender and somewhat winding stream of a white colour, that seem'd to flow down just like a Liquor from the depending drop, till it had reach'd the Spirit of *Sal Armoniack*; upon whose surface it would spread it self like a Mist. But this only upon the by. As for the main Experiment it self, it may be, as I have found, successfully try'd with other Liquors than these; but 'tis not necessary in this place to give an account of such Tryals; though perhaps, if I had leisure, it might be worth while to consider, whether these

These Coalitions of differing sorts of Steams in the Air, and the Changes resulting thence of their particular precedent Quantities, may not assist us to investigate the causes of divers sudden Clouds and Mists, and some other Meteorological *Phænomena*, and also of divers changes that happen in the Air in reference to the coming in and ceasing of several either Epidemical or contagious Diseases, and particularly the *Plague*, that seem to depend upon some occult temperature and alterations of the Air, which may be copiously impregnated by the differing subterranean (not to add here, Sidereal) Effluvia, that not infrequently ascend into it (or otherwise invade it,) with Pestiferous or other Morbifick Corpuscles, and sometimes with others of a contrary Nature, and sometimes too perhaps, neither the one sort of Steams, which may be suppos'd to have imbued the Air, is in it self deleterious; nor the other salutary, but becomes so upon their casual coalition in the Air. You will

will perhaps think this Conjecture of the resultancy of pestilential Steams the less improbable, if I here add that odd Observation, which was frequently made in the formerly mentioned Plague at *Nimwegen* by a Phy-

* *Tract. de Peste,*
lib. 2. cap. 3.

fician so Judicious as
* *Dimmerbrook*; whose words are these; *Illud*

notatu dignum sæpius observavimus nempe in illis edibus in quibus nulla adhuc pestis erat, si linteamina sordida aqua & sapone nostrate (ut in Belgio moris est) illic lavarentur, eo ipso die, vel interdum postridie, duos tres-ve simul peste correptos fuisse, ipsique egri testabantur factorem aquæ saponatæ illis primam & maximam alterationem intulisse. Hoc ipsum quoque in meo ipsius hospitio infelix experientia docuit, in quo post lota linteamina statim gravem alterationem perceperunt plerique domesticæ, & proximè sequenti nocte tres pesti correptæ, ac brevi post mortuæ fuere

I omit the Instances he further set down to confirm this odd Phenomenon, of which, though perhaps some other

ther Cause may be devised ; yet that lately assign'd seems at least a probable one, if not the most probable ; hence, as 'tis manifest by daily experience, that the smell occasion'd by the washing of foul Linnen with the soap commonly used in the *Netherlands*, produces not the Plague ; so by our Learned Author's Observation appears, either that there were not yet any Pestilential Effluxions in the Air of those places, which on the occasions of those washings became infected, or at least that by the addition of the fetid *Effluvia* of the soapy Water, those Morbifick Particles, that were dispers'd through the Air before, had not the power to introduce a malignant constitution into the Air, and to act as truly Pestilential, till they were enabled to do so by being associated with the ill-scented *Effluvia* of the Soap.

Whether also Salutary, and, if I may so call them, Alexipharmacal Corpuscles may not be produc'd in the Air by Coalition, might be very well

well worth our Enquiry : Especially if we had a competent Historical Account of the yearly ceasing of the Plague at *Grand Cayro*. For, as I have elsewhere noted out of the Learned *Prosper Alpinus*, who practis'd Physick there ; and as I have also been inform'd by some of my Acquaintance who visited that vast City, that almost in the midst of Summer as

* *The Plague which here miserably rageth upon the first of the Flood doth instantly cease ; in so much as when five Hundred dye at Cayro the day before , which is nothing rare, (for the sound keep company with the sick, holding Death fatal, and, to avoid them, Irreligion,) not one doth dye the day following ; says Mr. Sandys in his Travels, Lib. 2.*

soon as the River begins to rise *, the Plague has its malignity suddenly check'd , even as to those that are already infected, and soon after ceases ; so if other Circumstances contradict not, one might guess, that this strange *Phenomenon* may be chiefly occasion'd by some Nitrous or other Corpuscles that accompany the overflowing *Nile*, and by associating themselves with what *Hippocrates*

rates somewhere calls *vorreg's* *Σαππίας*,
 lifable them to produce their wonted
 pernicious Effects. To which Hy-
 pothesis fuits well what is deliver'd
 by more than one Traveller into
Egypt, and more particularly by our
 ingenious Countreyman Mr. *George*
Sandys, who not only takes notice,
 that about the time of the overflow-
 ing of *Nilus*, whose abounding with
 Nitre has been observed even by the
 Antients, there is a certain moisten-
 ing Emanation dif-
 fus'd thorow the Air. * *Mr. Sandys in*
 To prove, sayes he*, *the Book above-*
 speaking of the over- *ciud.*
 flowing of *Nilus*, that it proceedeth
 from a natural Cause, this one, though
 strange, yet true Experiment will
 suffice. Take of the Earth of *Egypt*
 adjoining to the River, and preserve
 it carefully, that it neither come to
 be wet nor wasted, weigh it daily,
 and you shall find it neither more nor
 less heavy until the seventeenth of
June, at which day it beginneth to
 grow more ponderous, and aug-
 menteth

menteth with the augmentation of the River, whereby they have an infallible knowledge of the state of the Deluge, proceeding without doubt from the Humidity of the Air, which having a recourse through all passible places, and mixing therewith increaseth the same, as it increaseth in moisture.

That these Sanative Steams perform their Effects meerly because they are moist, I presume Naturalists will scarce pretend; but that they may be of such a nature as by their Coalition with the Morbifick Corpuscles to increase their Bulk and alter their Figure, or precipitate them out of the Air, or clog their Agility, or pervert their Motions, and in a word destroy all or some at least of those Mechanical Affections which made those Corpuscles Pestilential: That, I say, these Antidotal Vapours (if I may so call them) may have these Effects upon those that formerly were Morbifick, and that so there may result from the Association of

two sorts of Particles, whereof one was of a highly noxious nature, a harmless mixture, might here be made probable by several things; but that I hope what I have lately recited about the Coalitions of the *Effluvia* of Spirit of Salt and of Urine (Liquors known to be highly contrary to each other) is not already forgotten by you.

And the Experiment with which I am to conclude this Essay will perhaps make you think it possible, that the Pestiferous Steams that have already pass'd out of the Air, and inhaled, but not too much vitiated, the Bodies of Men, may have their malignity much debilitated by the supervening of these Antidotal Particles. For in that Experiment you will find, that the Steams emitted into the Air from the Liquor there described, though that were actually cold, were able to reach, and manifestly to Operate, (and that probably by way of Præcipitation,) upon Coruscles that were sent from them
by

by the Interposition of other Bodies, not more porous than those of living Men. Whether the fume of Sulphur, which by many is extoll'd to prevent the Infection of the Air, do by its acid or other Particles disarm, if I may so speak, the Pestilential ones, I have not now time to inquire: No more than whether in *Ireland* and some few other Countries, that breed or brook no poysonous Animals, that hostility may proceed, at least in great part, from the peculiar Nature of the Soyl, which both from its superficial and deeper parts, constantly supplies the Air with Corpuscles destructive to venemous Animals. And some other Particulars, that may be pertinently enough consider'd here, you may find treated on in other Papers. And therefore at present I shall only intimate in a word, that having purposely made a visible and lasting Stain on a solid Body barely by cold *Effluvia*, I did by the invisible and cold Steams of another Body make in two or three Minutes a
visible

visible change in the colour of that Stain.

And as for the other part of the Conjecture, (*viz.*) That Meteors may sometimes be produc'd by the Occursions of Subterranean *Effluvia*, some of them of one determinate Nature, and some of another, I think I could, to countenance it, give you divers Instances of the plentiful Imregnation of the Air at some times, and in some places, with Steams of very differing Natures, and such as are not so likely to be attracted by the Heat of the Sun, as to be sent off from the Subterranean Regions, and sometimes from Minerals themselves. But for Instances of this kind, I shall, for brevities sake, refer you to another Paper*,

where I have particularly treated of this subject, and particu-

* *An Essay of Subterranean Exhalations.*

larly shewn, That though usually the Effluxions that come from under ground are ill-scented, yet they are not alwayes so; and also that Sulphureous

phureous Exhalations even from cold, and, for the most part, Aqueous Liquors may retain their determinate nature in the Air, and act accordingly upon solid Bodies themselves, to whose Constitution those *Effluvia* chance to be proportionate.

But one memorable Story not mention'd in that Discourse is too much to our present purpose to be here omitted, especially having met with it in so approved an Author as the experienc'd *Agricola*, who having mention'd out of antient Historians the Raining of White and Red liquors, which they took (erroneously I doubt

* *Agric. de Nat. eorum*
quæ effluunt e Terra,
Lib. 12. pag. 236.

not) for Milk and Blood, subjoyns,

* *Ut autem majorem*
fidem habeamus, An-

nalium monumentis facit res illa decantata, quæ Patrum memoria! (in another place he specifies the Year of our Lord in Suevia accidit; *Aer enim ille stillavit guttas, quæ lineas vestes crucibus rubri quasi sanguineis imbuebant.* Which the rather mention, because it does

not

not only prove what I alledge it for ;
but may keep , what is lately and
véry credibly reported to have hap-
pen'd in divers places of the King-
dom of *Naples* soon after the Fiery
Eruption of *Vesuvius* , from being
judg'd a *Phénoménon* either altogether
fabulous , (as doubtless many have
thought it,) or a Prodigie without
all example , as is presum'd even by
those that think it not miraculous.
And to this I add, that 'twill be the
less improbable , that the more agile
Corpuscles of Subterraneal Salts ,
Sulphurs and Bitumens, may be rais'd
into the Air, and keep distinct natures
there , if so fixt a Body as common
Earth it self can be brought to swim
in the Air. And yet of this the
worthy Writer newly quoted gives
us, besides what Annals relate, this
Testimony upon

his own know- * *Agric. de Nat. eorum*
edge: * *Certè hîc* quæ è Terra effluunt ,
Tempnicii undeci- Lib. 12. pag. 263.

num abhinc annum mense Septembri
fluxerunt imbres , sic cum terra lutea

commisti, ut eâ passim placere scilicet stratas viderem conspersas.

And to shew you that in some cases the Particles even of Vegetable Bodies may not so soon perish in the Air as they vanish there, but may retain distinct natures at a greater distance, than one would think, from the Bodies that copiously emit them; I shall add, that having desir'd an ingenious Gentleman, that went on a considerable Employment to the *East-Indies*, to make some Observations for me in his Voyage; he sent me among other things this Remarque: That having sayl'd along the Coast of *Ceylon*, (famous for Cinnamon-trees and well-scented Gums,) though they Coasted it almost a whole day, the Wind, that then chanc'd to blow from the shoar, brought them a manifestly odoriferous Air from the Island, though they kept off many miles (perhaps twenty or twenty-five) from the shoar. Nor should this be thought incredible, because the diffusion seems so disproportionate to
that

that of other Bodies dissolved by Fluids; as, for instance, though Salt be an active Body and resolvable into abundance of minute Particles, yet one part of Salt will scarce be tastable in an hundred parts of Water. For sensibly to affect so gross an Organ as that of our Taste, there is usually required in sapid Particles a bigness far exceeding that which is necessary to the making Bodies fit Objects for the sense of Smelling, and, which is here mainly to be considered, there is a great difference between the power. A Body has to impregnate so thin and fine a Fluid as Air, whose parts are so rare and lax, and that which it has to impregnate Liquors, such as Water or Wine, whose parts are so condensed as to make it not only visible and tangible, but ponderous. On which occasion I remember that having had a Curiosity to try how far a sapid Body could be diluted without ceasing to be so, I found by Cryal, that one drop of good Chymical, and, as Artists call it, Essential

Oyl of Cinnamon being duly mix'd by the help of Sugar with Wine, retain'd the determinate tast of Cinnamon, though it were diffus'd into near a quart of Wine. So that making a moderate estimate, I concluded, that upon the common supposition, according to which a drop is reckon'd for a Grain, one part of Oyl had given the specifick Tast of the Spice, it was drawn from, to near fourteen thousand parts of Wine. By comparing which Experiment with what I noted about the proportion of Salt requisite to make Water tast of it, you will easily perceive; that there may be a very great difference in point of diffusiveness between the little Particles that make Bodies sapid: Which may serve to confirm both some part of the *first Chapter* of the foregoing Essay of the Subtilty of *Effluvia*, and what I was lately saying to shew it possible, that Antimonial Glass might impart store of Steams to the Emetic Wine, without appearing upon common Scales to have lost of its weight

weight; since we see, that one Drop of so light a Body as Oyl may communicate not insensible *Effluvia*; but tastable Corpuscles to near a Quart of Liquor. But this is not all for which I mention our Experiment: for I must now add, that besides the almost innumerable Sapid parts of a spicy Drop communicated to the Wine, it thence diffused a vast number of odorous Particles into the Air; which both I, and others perceived to be imbued with the distinct scent of Cinnamon, and which perhaps the Liquor would have been found able to have Aromatized for I know not how long a time, if I had had leisure to prosecute the Observation.

CHAP. V.

THE third and last way I shall mention of shewing the Determinate Nature of *Effluvia*, is to

be taken from the Consideration of their Effects upon other Bodies than the Organs of our Senses ; (for of their Operations upon these we have already spoken in the foregoing *Chapter.*) For the Effects , that certain Bodies produce on others by their Effluviūms , being constant and determinate , and oftentimes very different from those , which other Agents by their Emissions work upon the same and other subjects , the distinct nature of the Corpuscles emitted may be thence sufficiently gather'd.

We may from the foregoing Tract of the Subtilty of *Effluvia* , borrow some Instances very pertinent to this place. For the temporary benumbedness or stupefaction , for example,

* See the Essay of the Subtilty of Effluviūms, Chap. 4.

produc'd in the Fisherman's Foot by the *Effluvia* * of the Fish (*Amoreatim*) mention'd by the Ingenious *Piso* , manifests , that those stupifying Emanations retain'd a peculiar and venomous nature during their whole passage through

through the Shoe, Stocking and Skin, interpos'd betwixt the Fish and the nervous part of the Foot benumb'd by it. And though there are very few other Bodies in the World, that are minute enough to pass through the pores of Glafs, 'tis apparent, by the Experiment there recited of the long Iron Hermetically seal'd up in a Glafs-pipe, that the Magnetical *fluvia* of the Earth may retain their peculiar and wonderful nature in a smallness that qualifies them to pass easily through the pores of Glafs it self.

But that I may neither repeat what you have already met with in the foregoing Tract, nor anticipate what I have to say in the next; I will employ in this *Chapter* some Instances that may be spar'd from both. That divers Bodies of a Venemous nature may exercise some such Operations upon others by their *Effluvia* transmitted through the Air, they are wont to do in their gross substance, is a Truth, whereof though I have

I have not met with *many*, yet I have met with *some* Examples among Physicians.

The Learned * *Sennertus* observes as a known thing, that the Apprentices of Apothecaries have been cast into profound Sleeps, when in distilling *Opiat* and *Hypnotick* Liquors they have received in at their Nostrils the Vapours exhaling from those Bodies.

'Tis recorded by the * Writer about Poysons, that the root and juyce of *Mandragora* casts those that take it, into deep *Sopor* not unlike a Lethargy. Although the Apples of the same Plant be thought to be much less malignant; yet *Levinus Lemnius* relates that it happen'd to him more than once, that having laid some *Mandrake* Apples in his Study, he was by their Steams made so sleepy, that he could hardly recover himself; but the Apples being taken away he regain

* In *Explicatione Herbarum Biblicarum*, cap. 2.

* *Lib. 6. parte 7. cap. 1.*

regain'd alacrity, and threw off all
lowness.

Among all Poysons there is scarce
any whose *Phænomena* are in my opi-
nion more strange than those that
proceed from a mad Dog; and yet
even this Poyson, which seems to
require Corpuscles of so odd and de-
terminate a nature, is recorded by
Physicians to have been conveyed by
Exhalations. *Aretæus* writes (as a
learned modern quotes him,) *Quòd*
rabido cane, qui in faciem, dum spiri-
us adducitur, tantummodò inspiraverit,
nullo modo momorderit, in rabiem
mo agatur. And as there are rela-
tions, among Physicians, of Animals,
that have become *Rabiosi* by having
eaten of the parts or excrements of
mad Animals; so

Cælius Aurelianus, * *Libro 3. Acutor.*
Morbor. who writes, that some

have been made to run mad, not by
being bitten, but wounded only with
the Claws of a mad Dog, tells us
also of a man, that fell into a *Hydro-*
mania (which is wont to be a high
degree

degree of the *Rabies*, and by some of the antienter Writers was employ'd to signifie that Disease) without being bitten by a mad Dog, but infected *solo odore ex rabido cane attracto*. By which Odours in this and other Narratives of Poysons I understand not a bare Scholastick *species*, but a swarm of *Effluvia*, which most commonly are all or at least some of them odorous. And though it may justly seem strange to many, that the Venom of a mad Dog should be communicated otherwise than by biting, which is suppos'd to be the only way he can infect by, it may appear less improbable, because *Mattheus de Gradibus* names a person, who, he says, prov'd infected after many days, by only having put his Hand into the Mouth of a mad Dog, who did not bite him. And the formerly mentioned *Matthiolus* relates, that he saw two, that were made rabid without any wound by the flabber of a mad Dog, with which they had the misfortune to be besmear'd.

* *Sennertus* himself affirms of a Painter of his acquaintance, that, when he had open'd a Box, in which he had long kept included *Realgar*; a noxious Mineral, sometimes used by Painters and not unknown to Chymists, and had unfortunately snuff'd the Steams of it, he was seiz'd with giddiness in his Head and faintings, his whole Face also swelling, though by taking of Antidotes he escap'd the danger.

* *Sennert. Libr. 6. part. 6. cap. 2.*

Divers other Examples we have met with in the writings of Physicians, which I forbear to add to these, because, I confess, I very much doubt the Truth of them, though the deliverers of some of them be men of note. But the probability of most of the things already cited out of credible Authors may be strengthened by what I shall now subjoyn, as a further proof of the distinct Nature of *Effluvia*; of which it will be a very considerable Proof, if Medicines, which are of a milder and more

more familiar nature and operation than Poysons, shall yet be able in some cases to retain, in their invisible Particles swimming in the Air, the same, (though not so great) power of Purg'ing, which is known to belong to them when their gross Body is taken in at the Mouth. Of this I have elsewhere, on another occasion, given some Examples. To which I shall now add, that I know a Doctor of Physick, that is usually Purg'd by the Odours or Exhalations of a certain Electuary, whose Cathartick Operation, when it is taken in substance is wont to be but languid. And another Doctor of my acquaintance causing good store of the root of black Hellebore to be long pounded in a mortar, most of those, that were in the room, and especially the party that pounded it, were thereby purg'd and some of them strongly enough. And the Learned *Sennertus* somewhere affirms, that some will be purg'd by the very Odour of *Colocyntida*. And 'tis not to be pass'd by

unre

unregarded, that in the cases I have
 call'd, Exhalations, that are en-
 dow'd with Occult Qualities, (for
 those of Cathartick Medicines are
 reckon'd among such) ascend into
 the Air without being forc'd from
 the Bodies they belong'd to by an
 External heat.

And if I would in this place al-
 ledge Examples of the Operations of
 such *Effluvia*, as do not pass into the
 Air, but yet operate only by the con-
 tact of the External parts of the Bo-
 dy, I could give Instances, not only
 of the *Purgative*, but the *Emetick* Qua-
 lities of some Medicines exerted with-
 out their being taken in at the Mouth,
 or injected with Instruments.

There are also other sorts of Exam-
 ples than those hitherto mentioned,
 that argue a Determinate Nature in
 the Effluxions of some Bodies emitted
 to the Air. Approv'd Writers tell
 us, that the Shadow of a Walnut-tree
 with the Leaves on it is very hurtful
 to the Head; and some Instances
 they give us of great mischief it has
 some-

sometimes done. And though the Shadow, as such, is not likely to be guilty of such bad Effects; yet the *Effluvia* of the neighbouring Plant may be noxious enough to the Head. For I, that was not at all prepossess'd with an opinion that it was so, and therefore without scruple resorted to the Shade of Walnut trees in a hot Countrey, was by experience forc'd to think it might give others the Head-ach, since it did to me, who; thanks be to God, both was, and am still very little subject to that distemper. And this brings into my mind an Observation that I have met with among some ingenious Travellers in to the *West-Indies*, who observe in general, and of late a Countrey-man of our own affirms it in particular, of the poysonous *Manchinello-tree*, that Birds will not only forbear to eat of the Fruit of venemous Plants, but, as to some of them, will not so much as light on the Trees: Which I therefore mention, because probably Nature instructs them to avoid such

Such Trees by some noxious Smell, or other Emanation, that offends the approaching Birds. And I remember, that some of our Navigators give it for a Rule to those that happen to land in unknown Islands or Coasts, that they may venture to eat of those parts of Fruits which they can perceive, the Birds, like kind Tasters, to have been pecking at before.

Nicolaus Florentinus (cited by *Sennertus*) tells us of a certain *Lombard*, that having in a House, that he nam'd, at *Florence*, burn'd a great black Spider at the flame of a Candle, so un-
naturally, that he drew in the Steams of it at his Nostrils, presently began to be much disorder'd and fell into a fainting fit, and for the whole Night had his Heart much disaffected, his Pulse being so weak, that one could scarce perceive he had any; though afterwards he was cured by Treacle, Diamosc, and the powder of Zedoary mixt together.

And I remember, that being some years ago in *Ireland*, I gather'd a
E certain

certain Plant (peculiar to some parts of that Countrey) which the Natives call *Maccu-buy*, because of strange Traditions that go about it; the chief of which I found by tryal not to be true: But yet being satisfied, that its Operations were odd and violent enough, I was willing to gratifie the chief Physician of the Countrey, who was desirous I should propose to him some wayes of correcting it; and whilst I was speaking of one that required the pounding of it, he told me on that occasion, that intending to make an extract of it with Vinegar, he caus'd his man to beat it well in a Mortar, which the man soon repented he had begun to do: And the Doctor himself, though at a pretty distance off, was so wrought upon by the Corpuscles that issued out into the Air, that his Head, and particularly his Face, swell'd to an enormous and disfiguring bulk, and continued tumid for no inconsiderable time after.

I have not leifure to subjoyn many more

more Instances to shew the Determinate Nature of Effluvia, small enough to wander through the Air, nor perhaps will it be necessary, if you please but to consider these two things. The *first*, that many odoriferous Bodies, as Amber, Musk, Civet, &c. as they will, by the adhesion of their whole substance, perfume Linens, Linnen, &c. so they will in some perfume some Bodies disposed to admit their action, though kept at a distance from them. And the *other* is, that in Pestilential Fevers and divers other Contagious sicknesses, as the Plague, Small-pox, or Measles, the same determinate Disease is communicable to sound persons; not only by the immediate contact of the infected party; but without it, by the Contagious Steams that exhale from his Body into the Air. And having said this and desir'd you to reflect upon it, I shall conclude this Chapter with an Experiment, that possibly will not a little confirm a great part of it.

Considering then with my self how I might best devise a way of shewing to the very Eye, That *Effluvia* elevated without the help of Heat, and wandering in the Air, may both retain their own Nature, and upon determinate Bodies produce Effects, that a Vulgar Philosopher would ascribe to Occult Qualities I remember'd, that I had found by tryals (made to other purposes) that Volatile and Sulphureous Salts would so work upon some Acid ones sublim'd with Mercury, as to produce an odd diversity of Colours, but chiefly an Inky one; on which account I judg'd it likely that my aim would be answer'd by the following Experiment.

I took an Ounce, or better, of such a Volatile Tincture of Sulphur,

* The Liquor here mention'd is, for the main, the same with that described by the Author in his Book of Colours, Experiment the

I have elsewhere taught you to make of Quick-lime, Sulphur and *Sal Armoniack*, and stop'd it up in a Vial capab

capable of containing at least twice as much; then taking a Paper whereon something had been written with invisible Ink, I laid it down six Inches off of the Vial, which, being unstop'd, began, upon the access of the Fire, to emit white Fumes into it, and by these, what was written upon the Paper, notwithstanding its distance from the Liquor, quickly became very legible, though not quite so suddenly, as if a Paper, written with the same clear Liquor, were held at the like distance directly over the orifice of the Vial. And having caus'd several pieces of clean Paper to be written on, with a new Pen dip'd in the clear Solution of Sublimate made in Water, 'twas pleasant to see, how divers of the Letters of several of these Papers, being plac'd within some convenient distance of the Vial, would be made plainly legible, and some of them more, some less blackish, according to their distances from the smoaking Liquor, and other Circumstances. But 'twas

more surprizing to see, that when I held or laid some of these Papers, though with the written side upwards, just upon or over the orifice of the Vial, though the contained Liquor did not by some Inches reach so high, yet the latent Letters would become not only legible but conspicuous in about a quarter of a Minute of an Hour (measur'd by a good Watch fit for the purpose, as more than one tryal assur'd me.) And as it may be observ'd, that in some Circumstances the smoaking Liquor and the Solution of Sublimate will make an odd Precipitate almost of a silverish colour, so in one or two of our Tryals we found a like colour produc'd by the Steams of that Liquor, in some of the colourless Ink. Nor is it so necessary to employ a visibly smoaking Liquor for the denigrating of invisible Ink at a distance. For I have to that purpose, with good success though not equal to that I have recited, employ'd a couple of Liquors wherein there was neither Sulphur
no

nor *Sal Armoniack*, nor Sublimate. What other Tryals I made with our Volatile Tincture of Sulphur, 'tis not necessary here to relate; only one Experiment, which you will possibly think odd enough, I shall not omit; because it will not only confirm the precedent Tryals, but also much of the foregoing Essay, by shewing the great Subtilty and penetrating power of Effluvia that seem rather to issue out very faintly, than to be darted out with any briskness.

Causing then something to be written with dissolv'd Sublimate upon a piece of Paper, we folded the Paper with the written side inwards, and then inclos'd this in the midst of six sheets of Paper, laid one upon another, not plac'd one within another, and folded up in the form of an ordinary Letter or packet to be seal'd, that, the edges of the enclosing Paper being inserted one within the other, the Fumes might not get into this written Paper but by penetrating through the Leaves themselves: This

done, that side of the Packet, on which there was no commissure, and on which, were it to be sent away, the Supercription should be written, was laid upon the orifice of the Vial, which (as was before intimated) was some Inches higher than the surface of the Liquor, and left there about ten Minutes; after which taking off the folded Papers, and opening them, we found, that the Steams had pervaded all the Leaves, in which the written Paper had been enclos'd. For, though the Leaves did not appear stain'd or alter'd, yet the formerly latent Characters appear'd conspicuous. I have not time to discourse, whether and how far this Experiment may assist us to explain some odd Effects of Thunder, or of that strange *Phænomenon*, (glanc'd at in the foregoing Chapter,) which is said to have happen'd lately in the Kingdom of *Naples* after the great Eruption of *Vesuvius*, which is said to have been follow'd by the appearing of the Crolles formerly mention'd, some of which

which have been found on the innermost parts of Linnen; that had been carefully folded up. But of these and the like things, I say, I have now no time to discourse, whether any thing derivable from our Experiment may be pertinently apply'd to their Explication. For which reason I shall add no more than that afterwards for further tryal we took a printed Book, that chanc'd to be at hand, and which we judg'd the fittest for our purpose, because the leaves being broad they might the better preserve a small Paper to be plac'd in the mid'st of them from being accessible to the Exhalations sidewise, and having put the design'd Paper into this Book, and held it to the orifice of the Vial, though there were no less than twelve leaves between them; yet those Letters, that happen'd to be the most tightly plac'd, were made inky in the short space of three Minutes at the utmost; though this Liquor had been so long kept and so often unstop'd to try Conclusions with it, that

that it had probably lost a good part
of the most spirituous and piercing
Particles.

NEW
EXPERIMENTS,
To make the PARTS
OF
FIRE and *FLAME*
Stable & Ponderable.

BY
The Honorable Robert Boyle.



LONDON:

Printed by WILLIAM GODDARD,
for *Moses Pitt*, at the Sign of the
White Hart in *Little Britain*. 1673.

NEW
EXPERIMENTS
TO DETERMINE THE FORCE
OF
AIR AND FLAME
IN THE COMBUSTION OF
GASES

BY
THE HON. JOHN DOUGLAS



LONDON:
Printed by J. JOHNSON, in Pall Mall.
1799.



A

P R E F A C E ;

S H E W I N G

The Motive, Design, and Parts
of the ensuing Tract.

THE Inducements which put me
upon the Attempt, express'd in
the Title of this Essay, were
hiesly these:

First, I consider'd, that the Inter-
tellar part of the Universe, consisting of
Air and Æther, or Fluids analogous to
one of them, is diaphanous; and that the
Æther is, as it were, a vast Ocean,
wherein the Luminous globes, that here
and there like Fishes swim by their own
motion; or like Bodies in whirlpools are
carried about by the Ambient, are but
very thinly dispers'd, and consequently
not the proportion, that the Fixt Stars
and

The P R E F A C E.

and Planetary Bodies bear to the diaphanous part of the world, is exceeding small and scarce considerable; though we should admit the Sun and Fixt Stars to be Opacous Bodies upon the account of their terminating our sight: which diffident Expression I employ, because I have elsewhere shewn by two or three Experiments, purposely devised, that a Body may appear opacous to our Eyes, and yet allow free passage to the beams of Light.

I further consider'd, that there being so vast a disproportion between the diaphanous part of the world and the Globes about which 'tis every way diffused, and with which it is sometimes in great portions mingled, as in the water, which together with the Earth makes up the Globe we inhabit; and the nature of Diaphanous Bodies being such, that, when the Sun or any other Luminous Body illustrates them, that which we call Light does so penetrate and mix it self per minima with them, that there is no sensible part of the transparent Body unlightned; I thought it worth the enquiry, whether a thing, so vastly diffuse

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as Light is were some thing Corporeal or not? And whether, in case it be, it may be subjected to some other of our Senses besides our Sight, whereby we may examine, whether it hath any affinity with other Corporeal beings; that we are acquainted with here below?

I did not all this while forget, that the Peripateticks make Light a meer Quality, and that Cartesius ingeniously endeavours to explicate it by a modification of Motion in an Ætherial matter: but I remember'd too, that the Atomists of old, and of late the Learned Gassendus, and many other Philosophers assert Light to be Corporeal; and that some Years since, though I declined to pass my Judgment about the Question, yet I had employ'd Arguments, that appear'd plausible enough to shew, That 'twas not absurd to suppose, that the Sun, which is the First Star most known to us, might be a Fiery Body. And therefore doubting, whether the Corporeity of Light would be in haste determined by meer Ratiocinations, I thought it very well worth the endeavouring to try whether I could do any thing

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thing towards clearing the dispute of it by Experiments; especially being persuaded, that, though such an attempt should be ineffectual, it would but leave the controversie in its former State, without prejudicing either of the contending Hypotheles; and yet, if it should prove successfull, the consequences of it would be very great and useful towards the explicating of divers Phenomena in divers parts of Natural Philosophy, as in Chymistry, Botanicks, and (if there be any such) the allowable part of Astrologie. (Nor perhaps would it be impossible by the help of slight Theoretical alterations, to reconcile the Experiments, I design'd, to either of the above-mention'd Hypotheles, and so, as to the Explication of Light, to one another.)

To compass then, what I aim'd at I thought, 'twas fit in the first place to try, what I could do by the Union of the Sun-beams, they being on all hands confess'd to be Portions (as I may so speak of true and Celestial Light: And then I thought fit to try, what could be obtain'd from Flame; not only because
the

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that is acknowledg'd to be a Luminary, but because I hoped, the difficulties, I foresaw in the other Tryals, might be in some measure avoided in those made with Flame; and if both sorts of them should succeed, the later and former would serve to confirm each other. According to the Method I proposed of handling these two Subjects, I should begin with some account of what I attempted to perform in the Sun-beams. But the truth is, that when I chanc'd to fall upon the Enquiry that occasion'd this Paper, besides that the time of the Year it self was not ever-favourable, the weather proved so extraordinary dark and unseasonable that it was wonder'd at; so that, though I was furnish'd with good Burning-glasses, and did several times begin to make tryals upon divers Bodies, as Lead, Quicksilver, Antimony, &c. yet the frequent interposition of Clouds and Mists did so disfigure my Attempts, that, however they were not all alike defeated; yet I could not prosecute the greatest part of them to my own satisfaction. And therefore being unwilling to build on them as yet;
F I shall

The PREFACE.

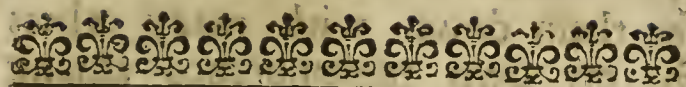
I shall reserve an account of them for another opportunity ; and now proceed to the mention of that sort of Experiments which depending less on Casualties, 'twas more in my power to bring to an Issue.

I know I might have saved both you and my self some time and pains by omitting several of these Tryals , and by a more compendious way of delivering the rest. But I rather chose the course I have taken ; partly because the Novelty and Improbabilities of the Truth I deliver seems to require , that it be made out by a good number of Tryals ; partly because I thought it might not be altogether useles to you and your Friends , to see upon what Inducements the several steps were made in this Inquiry ; partly because I was willing to contribute something towards the History that now perhaps will be thought fit to be made of the Increment or Decrement that particular Bodies may receive by being exposed to the Fire ; and partly (in fine , because the Incongruity of the Doctrine here asserted to the Opinions of the Schools , and the general Prepossession.

9

THE PREFACE.

of Mankind, made me think it fit by a considerable Variety, as well as number, of Experiments to obviate, as far as may be, the differing Objections and Evasions wherewith a Truth so paradoxical may expect to be encountred.



New EXPERIMENTS,

To make

FIRE and FLAME
PONDERABLE.

THough there be among the following Tryals a Diversity that invites me, as to rank them into four or five differing sorts, so to assign them as many distinct Sections; yet for the conveniency of making the References, there will be occasion to make betwixt them, I shall wave the Distinction, and set them down in one continued *Series*.

And because I am willing to comply with my host, as well as to deal frankly and without Ceremony with you, I shall venture to subjoyn the naked Transcripts of my Experiments, as I had in an artless manner

Experiments, to make

set them down with many others for my own remembrance among my *Adversaria*, without so much as retrenching some Circumstances that relate less to my present Argument, than to some other purposes.

I shall then begin with the mention of a couple of Experiments, which though they might conveniently enough be referr'd to another Paper; yet I shall here set them down; because it seems very proper to endeavour to shew in the first place, that Flame it-self may be as 'twere incorporated with close and solid Bodies so as to increase their bulk and weight.

Tryals

Tryals of the First sort.

EXPERIMENT I.

A Piece of Copper-plate not near so thick as a Half-crown, and weighing two Drachmes and twenty-five Grains, was so plac'd with its broad part Horizontal, in a Crucible, whose bottom had a little hole in it, for Fumes to get out at, that it could not be removed from its Position, nor be easily made to drop down or lose its Level to the Horizon, though the Crucible were turned upside down: Then about an Ounce and half of common Sulphur being put into a taller and broader Crucible, that, wherein the Copper stuck, was inverted into the orifice of it, that the Sulphur being kindled, the flame, but not the melted Brimstone in substance, might reach the Plate, and have some vent beyond it at the above-mentioned hole.

F 4

This

4 Experiments, to make

This Brimstone burn'd about two Hours, in which time it seem'd all to have been resolv'd into Flame, no flowers of Sulphur appearing to have sublimed into the inside of the upper Crucible; and though the Copper-plate were at a considerable distance from the ignited Sulphur, yet the Flame seem'd to have really penetrated it, and to have made it visibly swell or grow thicker; which appear'd to be done by a real accession of substance: since, after we had wip'd off some little adhering *sordes*, and with them divers particles of Copper that stuck close to them, the Plate was found to weigh near two and thirty Grains more than at first; and consequently to have increased its former weight by above a fifth part.]

EXPER. II.

[Having, by refining one Ounce of Sterling Silver with Salt-peter, according to our way reduc'd it to seven Drachms or somewhat less

FIRE and FLAME Ponderable. 5

we took a piece of the thus purified Silver, that weighed one Drachm wanting two Grains, and having order'd it as the Copper-plate had been in the former Experiment, after the flame of above one Ounce and a quarter of Sulphur, (that Quantity chancing to be suitable to the Capacity of the Crucible) had for about an Hour and a half beat upon it, the Silver-plate seem'd to the Eye somewhat swell'd, and the lower surface of it, that was next the flame, was brought to a great smoothness, the weight being increas'd to one Drachm five Grains and three quarters; which increase of weight falling so short of that which was gain'd by the Copper, I leave it to you to consider, *whether* the difference may be attributed to the closeness and compactness of the Silver, argued by its being heavier in *specie* than Copper; *or* to the greater congruity of the pores of Copper to be wrought on by the fiery *Menstruum*; *or* to some other cause.]

If

6 Experiments, to make

If you should here ask me, by what Rational inducements I could be led to entertain so extravagant an expectation, as that such a light and subtile Body as Flame should be able to give an augmentation of weight to such ponderous Bodies as Minerals and Metals; I shall now, to avoid making anticipations here, or needless repetitions hereafter, return you only this Answer: That the expectation you wonder at may justly be entertained upon the same or such like inducements, as you may easily discover in another Paper, entitled *Corollarium Paradoxum*. For, supposing upon the grounds there laid, that Flame may act upon some Bodies as a *Menstruum*, it seems no way incredible, that, as almost all other *Menstruums*, so Flame should have some of its own Particles united with those of the Bodies expos'd to its action: And the generality of those Particles being, (as 'tis shewn in the *Paradox* about the Fewel of Flames,) either Saline, or of some such

FIRE and FLAME *Wonderable.* 7

uch piercing and Terrestrial nature,
is no wonder, that being wedg'd
into the Pores, or being brought to
adhere very fast to the little Parts of
the Bodies expos'd to their action,
the accession of so many little Bodies,
that want not gravity, should, be-
cause of their multitude, be conside-
rable upon a Ballance, whereon one
two, or but few of these Corpuscles
could have no visible Effect.

I could here, if it were expedient,
mention some odd scruples about the
preceding Experiments, and some also
the subsequent; but, lest you should,
with some other of my Friends, up-
braid me with being too jealous and
sceptical, I will not trouble you
with them; but proceed to the next
set of Tryals, wherein, though the
matter were not always manifestly
eaten on by a *shining* Flame; yet
was wrought on by that, which
could be called Flame by those who
take not that *word* strictly, but in a
latitude, and which this Igneous sub-
stance may more properly be stiled,
than

8 **Experiments, to make**
than it can be call'd common Fire,
this being visibly harbour'd in burn-
ing Coals or other gross materials,
from which our Metals were fenc'd.
And I have elsewhere shewn by ex-
periment, that *Visibility* is not in al-
cases necessary to Actual Flame, par-
ticularly when the Eye receives a
predominant impression from ano-
ther Light.

Tryals of the Second sort.

EXPER. III.

INto a Crucible, whose sides ha-
been purposely taken down to
make it very shallow, was put one
Ounce of Copper-plates; and this
being put into our Cupelling-furnace
and kept there two Hours, and then
being taken out we weighed the Cop-
per (which had not been melted
having first blown off all the ashes)
and we found it to weigh one Ounce
and thirty Grains.

EXPER

EXPER. IV.

[Supposing that Copper, being educ'd to filings, and thereby gaining more of Superficies in proportion to its bulk, would be more expos'd to the Action of the Fire, than when it is in places as it was formerly; we took one Ounce of that Metal in filings; and putting them upon a very shallow Crucible, and under a Muffler, we kept them there about three Hours, (whilst other things that required so long a time were Cupelling;) and afterwards taking them off, we found them of a very dark colour, not melted but caked together in one Lump, and increas'd in weight (the ashes and dust being blown off) no less than about forty-nine Grains. Part of which increment, above that obtained by the Copper-plates in the former Experiment, may not improbably be due to the longer time that in this Experiment the Red Copper was kept in the Fire.]

EXPER.

EXPER. V.

[Being willing to see, whether *calcin'd Harts-horn*, that I did not find easie to be wrought on by corrosive *Menstruums*, would retain any thing of the Flame or Fire to which it should be expos'd; we weigh'd out one Ounce of small Lumps of Harts-horn, that had been burnt till they appear'd white, and having put them into a Crucible, and kept them in a Cupelling-furnace for two Hours whilst some Metals were driving off there by the violence of the Fire; we found, that when they were taken out, they had lost six or seven Grains of their former weight; perhaps *either* because, notwithstanding the external whiteness of the Lumps, the internal Parts of some of them might not be so exquisitely *calcin'd*, but retain some Oleaginous or other Volatile Substance; *or*, because, having omitted to ignite them well before they were weigh'd, they may have since

FIRE and FLAME Ponderable. 11

since their first Calcination imbib'd some moist Particles of the Air. Which conjecture seem'd the likelier, because, having kept them a while in the Scales they were weigh'd in, they did within two or three Hours make it somewhat preponderate. On which occasion I shall add, that, at the same time, with the Harts-horn we put in one Ounce of well-heated Brick, and kept that likewise in the Furnace for above two Hours; at the end of which weighing it whilst continued hot, we did not find it to have either sensibly got or lost; but, some time after, it seem'd upon the Ballance to have imbib'd some, though but very little, moisture from the Air.]

EXPER. VI.

[Upon a good Cupel we put one ounce of *English* Tin of the better sort, and having plac'd it in the Furnace under a Muffler, though it presently melted, yet it did not forsake its

its place, but remain'd upon the concave surface of the Cupel, till at the end of about two Hours it appear'd to have been well calcin'd; and then being taken out and weigh'd by itself, the Ounce of Metal was found to have gained no less than a Drachm.]

EXPER. VII.

[An Ounce of Lead was put upon the Cupel, made of calcin'd Hartshorn, and placed under the Muffler after that the Cupel was first made hot and then weigh'd. This Lead did not enter into the Cupel, but was turn'd into a pretty kind of Litharge on the top of it, and broke the Cupel, whereby some part of the Cupel was lost in the Furnace, and yet the rest, together with the Litharge, weigh'd seven Grains more than the Ounce of Lead and the heated Cupel did when they were put in.]

But because; though this tryal shew'd that some weight was gain'd either by the Metal or Cupel, or both;

yet

FIRE and FLAME Wonderable. 13

yet it did not by this appear, what either of them acquir'd; it seem'd fit to subjoyn a further tryal.

EXPER. VIII.

[We took a Cupel about two Ounces in weight, made of about ten parts of Bone-ashes, and one of Charcoal-ashes, made up together with Ale. This was by it self put in a Cupelling-furnace, under a Muffler; and the Laborant, well vers'd in weighing, was order'd to take it out when 'twas thoroughly and highly heated, and to weigh it whilst 'twas in that condition (I being then present:) This being done, 'twas forthwith plac'd again under the Muffler; where some Metalline Bodies were Cupelling, and kept there for about two Hours; at the end of which time 'twas taken out red-hot, and presently put into the same Ballance, before, which was already fastned to a Gibbet; where having caus'd the adhering ashes to be blown off,

G I found,

14 Experiments, to make

I found, that whereas, when 'twas first taken from under the Muffler, we had but two Ounces and two Grains, now the same weight being put into the opposite Scale, it had gain'd very near one and twenty Grains. And here note, that 'twas not without some cause, that I was careful to have the Cupel weighed red-hot. For I had a suspicion, that, notwithstanding the dryness of the Bone, it might receive some little alteration of weight by imbibing some little Particles wandering in the Air; which suspicion the event justified. For leaving the Cupel counterpois'd to cool in the Ballance, in a short time it began sensibly to preponderate; and suffering it to continue there nine or ten hours, till we had occasion to use the Ballance, I found it at the end of that time to be about three Grains heavier than before.]

This was not the only tryal we made about the augmenting the weight of Cupels; but this being the fairest, and exempt from those mis-

FIRE and FLAME Wonderable. 15

mischances, from which the other were not altogether free; I shall content my self to have set down this: In the mention of which I thought fit to take notice of the increase of the weight of the Cupel after it had layn in the Scales, and also that we weighed it at first whilst it was thoroughly hot, because those Circumstances, as not being suspected, may easily be left unthought on, even by skilful Experimenters; and yet the weighing of the Cupel, when it had been well cal'd, and the not weighing it soon enough after 'tis taken from the Fire, may keep those, that shall reiterate this Experiment, from making it cautiously and accurately enough. For if the former Circumstance be omitted, that which the Cupel may seem to have lost of its substance, is nothing but the adventitious moisture of the Air; and if the later Circumstance be neglected, the weight, it may seem to have gain'd from the Fire, was *indeed* due to the waterish Particles of the Air. I could

16 Experiments , to make

with also , that tryal were made , whether the success would be the same in Cupels made in differing sorts of Bone-ashes , and other materials , wont to be employed for that purpose. For That I had not opportunity to do.

EXPER. IX.

Iron being a Metal , that experience had inform'd me will more easily be wrought on by Fluids that have Particles of a Saline nature in them , than is commonly believed ; 'twas not unreasonable to expect , that Flame would have a greater Operation on it, (especially if it were before-hand reduc'd to small Parts) than on any of the Bodies hitherto describ'd. Which supposition will be confirm'd by the short ensuing Note

[Four Drachms of filings of Steel being kept two Hours on a Cupe under a Muffler, acquir'd one Drachm six Grains and a quarter increase of weight.]

EXPER

E X P E R. X.

[A piece of Silver, refin'd in our own Laboratory, being put upon a Cupel under a Muffler, and kept there for an hour and half, whilst other things were refining, was taken out and weigh'd again, and, whereas before it weighed three Drachms, thirty-two Grains and a quarter, it now weighed in the same Scales three Drachms, thirty-four Grains and a half, or but little less.]

Finding this Memorial among divers others about the Weight of Bodies, expos'd to the Fire, I thought it not amiss to annex it in this place; though finding it to be but single, would not have it to be rely'd on till further tryal have been made to discover, whether it was more than casual and anomalous Experiment; and if the Silver had not been refin'd, should have suspected, that the Copper, that was blended with it, 'tis usually blended with common

G 3 Silver,

18 Experiments, to make

Silver, might have occasioned the increase of weight.

(*Postscript.*)

Since the foregoing Experiment was first set down, meeting with an opportunity to reiterate the tryal once more, we did it with half an Ounce of filings of Silver, well refin'd with Lead in our own Laboratory, and kept it about three hours upon the Cupel; after the end of which time taking it out, we found it to be of a less pleasant colour than it was of before, and melted (though not so perfectly) into a Lump, which weigh'd four Drachms and six Grains; and yet, the success being so odd, and, if it prove constant, of such moment, I could wish the tryal were further repeated in differing quantities of the Metal.

EXPER. XI.

[We took a Drachm of filings of *Zink* or *Spelter*, and having put it upon

FIRE and FLAME Ponderable. 19

upon a Cupel under a Muffler, we kept it there in a Cupelling-fire about three Hours, (having occasion to continue the Cupellation so long for other tryals;) then taking it off the Cupel, we found it to be caked into a brittle and dark-colour'd Lump, which look'd as if the filings had been calcin'd. This being weigh'd in the same Scales gain'd full six Grains, and so a tenth part of its first weight.]

EXPER. XII.

Among our various tryals upon common Metals, we thought fit to make one or two upon a Metal brought us from the *East-Indies*, and there call'd *Tutenâg*, which name being unknown to our *European* Chymists, I have elsewhere endeavoured to give some account of the Metal itself; whence I shall borrow the ensuing Note, as directly belonging to our present purpose.

[Two Drachms of filings of *Tutenâg*

20 Experiments , to make

nâg being put upon a Cupel, and kept under the Muffler for about two hours, the filings were not melted into a Lump of Metal, but look'd as if Ceruse and *Minium* being powder'd had been mingled together; some of the parts appearing distinctly white, and others red: The *Calx* being put into the Balance appear'd to have gained twenty-eight Grains and a quarter. Another time the Experiment being reiterated with the like Circumstances, we found, that two Drachms of the filed *Tutenâg* gained the like increase of weight, abating less than one Grain.]

So that this *Indian* Metal seems to have gain'd more in the fire, in proportion to its weight, than any we have hitherto made tryal of.

EXPER. XIII.

[Being desirous to confirm by a clear Experiment, what I elsewhere deliver contrary to the vulgar Opinion of those that believe, that in all Cupellations almost all the Lead that

FIRE and FLAME Wonderable. 21

What is employ'd about them, does, together with the baser Metals that are to be purg'd off from the Silver or Gold, fly away in Smoak, as indeed in some sort of Cupellations a good proportion may be blown off that way: We took two Ounces of good Lead and one Drachm of filings of Copper, and having caus'd a Cupel to be ignited, and nimbly taken out of the Furnace, and weighed, whilst it was very hot, 'twas presently put back, together with the two Metals laid on it, into the Cupelling-furnace, where having been kept for about two hours, it was taken out again, and 'twas found, according to what (as I elsewhere * note) uses to happen in such Circumstances, to have nothing on the surface of it worth weighing distinct in the Scales, in which the Cupel with what was sunk into it amounted to four Ounces three Drachms and seven Grains, which wanted but one Grain of the whole weight of the

* *Essay the sixth
of the Useful, of
Nat. Philos.*

the Cupel and the two Metals, when they were all three together committed to the Fire.] So that, though we make a liberal allowance for the increment of weight that may with any probability be supposed to have been attained by the Cupel and what was put upon it, yet it will easily be granted, that very much the greater part of the Metals was not driven off in Fumes, but enter'd into the Substance of the Cupel.

Tryals of the Third sort.

After having shewn that either Flame or the Analogous Effluxions of the Fire will be, what Chymists would call, Corporified with Metals and Minerals exposed naked to its action; I thought it would be a desirable thing to discover, whether this Flame or igneous Fluid were subtil enough to exercise any such Operation upon the Light Bodies shelter'd from its immediate contact

FIRE and FLAME *Wonderable.* 23

contact by being included in close Vessels; but it being very difficult to expose Bodies in Glasses to such vehement Fires without breaking or melting the Glass, and thereby losing the Experiment; I thought fit, first to employ Crucibles carefully luted together, that nothing might visibly get in or out, and of that attempt find among my Notes the following Account.

EXPER. XIV.

[We took an Ounce of Steel freshly filed from a Lump of that Metal, that the filings might not be rusty, and having included them betwixt two Crucibles, as formerly, kept them for two hours in a strong Fire, and suffer'd them to continue there till the Fire went out; the Crucibles being unluted, the filings appear'd hard caked together, and had acquir'd a dark colour somewhat between black and blew, and were increas'd five Grains in weight.]

The

The foregoing Experiment being the first I mention of this kind, 'twill not be amiss to confirm it by annexing the following Memorial.

[An Ounce of filings of Steel being put between the Crucibles luted together, after they had been kept about an hour and half in the fire were taken out, and being weigh'd were found to have gained six Grains.

EXPER. XV.

[Two Ounces of Copper-plates were put into a new Crucible, over which a lesser was whelmed, and the commissures were closed with lute, that nothing might fall in. After the same manner two Ounces of Tin were included betwixt Crucibles, and also two Ounces of Lead these being put into the Cupelling furnace were kept in a strong Fire about an hour and a half, while something else was trying there. And then being taken out, the event was that the Copper-plates, though the
stuc

FIRE and FLAME Ponderable. 25

tuck together, were not quite melted, and seem'd some of them to have acquir'd scales like Copper put into a naked Fire, and the two Ounces had gain'd eight Grains in weight. The Lead had broke through the bottom of the Crucible, and thereby hinder'd the design'd Observation. The Tin acquir'd six Grains in weight; and was in part brought to a pure white *alx*, but much more of it was melted into a Lump of a fine yellow colour, most like Gold, but deeper.] The prosecution of this tryal as to the Copper-plates you will meet with *Experim. XXI.* to which I therefore refer you.

N. B. Because Lead in Cupellation enters the Cupel, we were willing to try, if we could so far hinder from doing so, as to make some estimate what change of Weight the operation of the Fire would make it: And therefore being able already to make a near guess, how much a quantity of Tin may gain by being calcined on a Cupel, and remem-

26 Experiments, to make

remembring also from some of my former tryals the indisposition which Tin gives Lead to Cupellation, we mixed a Drachm of Tin with two Ounces of Lead, and exposing the mixture (in a Cupel) to the Fire under a Muffler, we first brought it to fusion, and then it seem'd at the top dry and swell'd and discolour'd notwithstanding which, having continued the Operation a good while because of other things that were to be done with the same Fire, we were not lucky enough to bring the Experiment to an issue worth the relating here, in reference to the scope above-propos'd, though in relation to another the success was welcome enough.]

EXPER. XVI.

[Supposing that if Copper were beaten into thinner plates than those we lately us'd, and kept longer in the fire, this would have a more considerable Operation upon them, we took

FIRE and FLAME Powderable. 27

Took one Ounce of very thinly hammer'd pieces of Copper, and putting them betwixt two Crucibles (one helm'd over another) as in *Experiment. XV.* with some lute at the corners of the juncture, to keep the fire from coming immediately at the Metal, we kept them in the Cupelling-furnace about three hours, and then disjoyning the Vessels, we found the Metal covered with a dark and brittle substance, like that describ'd in the above recited Experiment. Which substance, when scal'd off, disclos'd a finely colour'd Metal, which, together with these burnt scales, amounted to one and twenty Grains above the weight that was first put in.]

If, when these things were doing, I had been furnished with a very good Lute, which is no such easie thing to procure, as Chymists, that have not frequently employed vulgar stoves, are wont to think; I would have made a tryal of the ensuing Experiment for a good while in the naked Fire, notwithstanding that
divers

28 Experiments, to make

divers Metalline Minerals will scarce be brought to fusion in Glasses, especially without such a Fire, whose violence makes them break the Vessels. For I thought, that by making a fit choice of the Metals to be employed, I could prevent that inconvenience: But wanting the Accommodations I desir'd, and yet presuming, that in a Sand-furnace I might by degrees administer heat enough to melt so fusible a Metal as fine Tin and keep it in fusion; I resolved to make some tryals, first upon that, and then upon another Metal. For though I was not sure of being then able to prosecute the Experiment far enough yet I hoped, I might at least see some Effects of my first tryal, which would enable me to guess, what I was to expect from a complete one.

EXPER. XVII.

[We took then a piece of fine Block-Tin, and in a pair of good Scales weigh'd out carefully half
Poun

FIRE and FLAME **Wonderable.** 29

Pound of it ; this we put into a choice Glass-retort , and kept it for two days or thereabouts in a Sand-furnace, which gave heat enough to keep the Metal in fusion without cracking the Glass. Then taking out the mixture, we carefully weigh'd it in the same Scales, and found the superficies a little alter'd (as if it were dispos'd to calcination) and the weight to be increased about two Grains or somewhat better.]

EXPER. XVIII.

[The other Experiment , I tryed in Glasses, was with Mercury, hoping, that , if I could make a Precipitate *in se* in a Hermetically seal'd Glass, should by comparing the weight of the Precipitate , and the Quick-silver that afforded it , have a clear experiment to my purpose ; and I could have no bad one , if I could but make it succeed with a Glass, though not seal'd , yet well stop'd ; instead of those Infernal-glasses (as
H they

30 Experiments, to make

they call them) which are commonly us'd and wont to be left open (though some slightly stop them with a little Paper or Cotton :) But though, partly that I might a little diversifie the Experiment, and make it the more likely to succeed in one or other of the Glasses, I divided the Mercury and distributed it amongst several of them, and but a little to each, the success did not answer expectation, the Hermetically seal'd Glasses being unluckily broken; and the Precipitation in the others proceeding so slowly, that I was by a remove oblig'd to leave the tryal imperfect; only I was encouraged, (in case of a future opportunity) to renew it another time, by finding that most of the Glasses, though tall, and stop'd with fit Corks, afforded some very fair Precipitate, but not enough to answer my Design.]

Tryals

Tryals of the Fourth sort.

Most of the Experiments hitherto recited, having been made as it were upon the by with others, whose exigencies 'twas fit these should comply with; very few of the expos'd Bodies were kept in the Cupelling-fire above two hours or thereabouts. Upon which account I thought fit to try, how much some Bodies, that had been already expos'd to the Fire, would gain in weight by being *again* expos'd to it; especially considering, that most calcinable Bodies, (for I affirm it not of *all*) which yield rather *calces* than ashes by being without additament reduc'd in the Fire to fine powder, seem'd to be that Operation open'd, or (as a Chymist would speak) unlock'd, and therefore probably capable of being further wrought upon and increas'd in weight by such a *Menstruum* as I suppos'd Flame and igneous Exhalations

32 Experiments, to make

halations to be. And about this Conjecture I shall subjoyn the ensuing Tryals.

EXPER. XIX.

[One Ounce of *Calx* of Tin, that had been made *per se* for an Experiment in our own Laboratory, being put in a new Cupel and kept under the Muffler for about two hours, was taken out hot and put into the Scales, where the powder appear'd to have gain'd in weight one Drachm and thirty-five Grains by the operation of the Fire, which made it also look much whiter than it did before as appeared by comparing it with some of the *Calx* that had not been expos'd to the second Fire: No part of the Puttie was, as we could perceive, melted by the vehemence of the Fire; much less reduc'd into Metal.]

EXPER

EXPER. XX.

[Out of a parcel of filings of Steel, that had been before expos'd to the fire, and had its weight thereby increas'd some Grains, not Scruples; we took an Ounce, and having expos'd it at the same time with the *Calx* of Tin, and, for the same time, kept it in the Fire, we took it out at the two hours end; and found the weight to be increas'd two Drachms and two and twenty Grains. The filings were very hard bak'd together, and, the Lump being broken, looked almost like Iron.]

EXPER. XXI.

The following Experiment, though may seem in one regard but a Continuation of the XVth; yet it has in this something peculiar from all the foregoing, that not only it affords an instance of the increase of Weight obtain'd by a Metal at the second

H 3 time

34 Experiments, to make

time of its being expos'd to the fire, but shews also, that such an increment may be had, though this second ignition be made in *close* Vessels.

[Some of the Copper mention'd in *Experim. XV.* being accidentally lost, one Ounce and four Drachms of what remain'd was included betwixt two Crucibles and expos'd to a strong fire for two hours, and suffer'd to continue there till the fire went out: When it was taken out it appear'd to have gain'd ten Grains in weight, and to have upon the superficial parts of the Plates (as we observ'd) divers dark colour'd flakes, some of which stuck to the Metal, but more, upon handling it fell off.]

And here I shall conclude One of the Two Parts of our designed Treatise: For, though I remember, that these were not all the Tryals that were made and set down upon the Subject hitherto treated of; yet these are the chief, that having escaped the mischances, which beset some others,

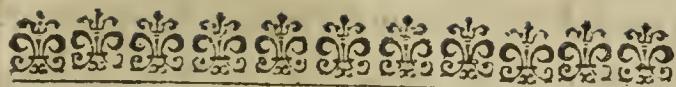
FIRE and FLAME Bonderable. 35

others, I can meet with among my promiscuous Memorials; whose number, when I drew them together, I could scarce increase, having by all these and other Tryals of differing kinds wasted my Cupels and commodious Glasses, where I could not well repair my loss. Whether I should have been able by Reduction, specifick Gravity, or any other of the ways, which I had in my thoughts, to make any discovery of the Nature of the Substance that made the Increment of Weight in our Ignited Bodies; the want as well of leisure, as of accommodations requisite to go through with so difficult a task, keeps me from pretending to know. But these three things, I hope, I may have gained by what has been delivered. The *First*, That we shall henceforth see cause to proceed more warily in the Experiments we make with Metals in the Fire, especially by Cupellation. The *next*, That it will justifie and perhaps procure an easier assent to some passages in my

36 Experiments, to make, &c.

other Writings, that have Relation to the Substance, what-ever it be, that we are speaking of. And the *third*, (which is the principal,) That it will probably excite you, and your inquisitive Friends, to exercise their sagacious Curiosity, in discovering what kind of Substance that is, which, though hitherto overseen by Philosophers themselves, and, being a Fluid, far more subtile than visible Liquors, and able to pierce into the Compact and Solid Bodies of Metals, can yet add something to them, that has no despicable Weight upon the Ballance, and is able for a considerable time to continue fixt in the Fire.

Addi-



Additional Experiments,

A B O U T

ARRESTING and WEIGHING
O F

Igneous Corpuscles.

EXperiments to discover the Increase in Weight of Bodies, though inclos'd in Glasses, being those that I considered as likeliest to answer what I design'd in the hitherto prosecuted Attempt, and finding the seventeenth *Experiment* as well as the next (try'd upon Mercury) to be very slow, and its performance not to be very great, I began to call to mind, what, many years ago, Experience had shewn me possible to be perform'd, as to the managing Glais-vessels, even without coating them, in a naked Fire, pro-

38 Additional Experiments.

provided a wary person were constantly employ'd to watch them And supposing hereupon, that, in no longer time than a Laborant might without being tir'd, hold out to attend a Glass, a Metal expos'd in it to a naked fire might afford us a much more prosperous tryal than that lately referr'd to, I afterwards resolv'd when I should be able to procure some Glasses conveniently shap'd, to prosecute my Design; in pursuance of which though I had not any Furnaces fitted for my purpose, I directed a Laborant to make the following Tryals.

EXPER. I.

[We took eight Ounces (Troy weight) of Block-Tin, which being cut into bits was put into a good round Vial with a long neck, and then warily held over quick Coals without touching them till it was melted; after which it was kept almost continually shaken, to promote the Calcination, near an hour, the Metal

Additional Experiments. 39

Metal being all the while in fusion, and the Glass kept at some distance from the thoroughly kindled Coals. The most part of this time the orifice of the Vial was cover'd with a Cap of Paper (which sometimes fell off by moving the Glass) to keep the Air and Steams of the Coals from getting into the neck. And at the end of this time, he that held the Glass being tir'd, and having his Hand almost scorch'd, the Vial being remov'd from the fire was broken, that we might take out the Metalline Lump, which had a little darkish *Calx* here and there upon the upper surface, but much more beneath, where it had been contiguous to the bottom of the Glass; then putting all this carefully freed from little fragments of broken Glass into the same Ballance with the self-same counterpoise I had us'd before, I found, according to my Expectation, an increase of weight, which amounted to eighteen Grains, that the Tin had acquir'd by this Operation.]

EXPER.

EXPER. II.

[This done we separated the *Calx* for fear of losing it, and having melted the Metal in a Crucible, that by pouring it out it might be reduced to thin Plates capable of being cut in pieces, and put into such another Vial as the last; we weigh'd it again together with the lately reserved *Calx*, but found, that, notwithstanding all our care, we had lost three Grains of the eighteen we had gain'd. This done we put the Metal into another Vial. But in regard the neck was shorter than that of the former, and could not like it be long held in ones Hand; and because also I was willing to see what Interest the shaking of melted Tin has in the quickness of the Calcination, the Glass, which had a stopple of Paper put to it to keep out Smoak and Air, was held at some distance from the Coals, only whilst the Tin was melting; and then was warily laid upon

Additional Experiments. 41

upon them and kept there for two hours, at the end of which 'twas again taken off, and the Metal weigh'd with the same Counterpoise and Balance as formerly; and then it appear'd to amount to eight Ounces twenty-four Grains, and to have much more separable *Calx* than at the first time. Nor did I much wonder, that the weight should be increas'd in this last Operation but nine Grains in two hours, and in the former twice so many in half the time; since, during the two hours, the Glass was kept in one posture, whereas in the first Operation, it was almost perpetually shaken all the while 'twas kept in fusion. And 'tis observ'd, that the agitation of melted Minerals will much promote the Effect of the Fire upon them, and conduce to their Calcination.]

EXPER. III.

Though these Tryals might well satisfy a person not very scrupulous,
yet

42 Additional Experiments.

yet to convince even those that are so, I undertook, in spite of the difficulties of the Attempt, to make the Experiment in Glasses Hermetically seal'd, to prevent all suspicion of any accession of Weight accruing to the Metal from any Smoak or Saline Particles getting in at the mouth of the Vessel. And in prosecution of this design I thought upon a way of so Hermetically sealing a Retort, that it might be expos'd to a naked fire without being either crackt or burst; an Account of which Tryal was thus set down.

[Eight Ounces of good Tin carefully weigh'd out was Hermetically seal'd up in a new small Retort with a long neck, by which 'twas held in ones Hand, and warily approach'd to a kindled Charcoal-fire, near which the Metal was kept in fusion, being also ever now and then shaken for almost half an hour, in which time it seem'd to have acquir'd on the surface such a dark colour as argued a beginning of Calcination, and it
both

Additional Experiments. 43

both emitted Fumes that play'd up and down, and also afforded two or three drops of Liquor in the neck of the Retort. The Laborant being not able to hold the Glass any longer; it was laid on quick Coals, where the Metal continued above a quarter of an hour longer in fusion; but before the time was come that I intended to suffer it to cool in order to the removing it, it suddenly broke in a great multitude of pieces, and with noise like the Report of a Gun; but thanks be to God) it did no harm either to me nor others that were very near it. In the neck we found some drops of a yellowish Liquor, which a *Virtuoso* that tasted it affirm'd to be of an odious but peculiar Savor; and as for the Smell, I found it to be very stinking, and not unlike that of the distill'd Oyl of Fish.] But, though our first Attempt of this kind had thus miscarried, we were not thereby discourag'd, but in prosecution of the same design made the ensuing Tryal.

EXPER.

EXPER. IV.

[The Tin which had been before (in the first or some such Experiment) partly calcin'd in a Glass, being melted again in a Crucible, that it might be reduc'd to pieces small enough to be put into another Glass was put again into the Scales, and the surplufage being laid aside, that there might remain just eight Ounces these were put into a Bolt-head of white Glass with a neck of about twenty Inches long, which being Hermetically seal'd (after the Glass had been a while kept over the fire lest that should break by the rarefaction of the Air,) the Metal was kept in fusion for an hour and a quarter as (being hinder'd by a Company of strangers from being there my self the Laborant affirm'd. Being unwilling to venture the Glass any longer, it was taken from the fire and when 'twas grown cold, the seal'd end was broken off; but before I wou

Additional Experiments. 45

I would have the bottom cut out, I observ'd, that the upper surface of the Metal was very darkly colour'd, and not at all smooth, but much and very odly asperated; and the lower part had between the bottom and the lower part of the Lump a pretty deal of loose dark-colour'd *Calx*, though the neighbouring surface and some places of the Lump it self look'd by Candle-light (it being then Night) of a golden Colour. The Lump and *Calx* together were weigh'd in the same Scales carefully, and we found the weight to have increas'd twenty-three Grains and better, though all the *Calx*, we could easily separate, being weigh'd by it self amounted not to four Scruples or eighty Grains.]

For Confirmation of this Experiment I shall subjoyn another, wherein at a quarter of so much Metal was employed with such success as the annexed Memorial declares.

I

EXPER.

46 Additional Experiments.

E X P E R. V.

[Two Ounces of filings of Tin were carefully weigh'd and put into a little Retort, whose neck was afterwards drawn slenderly out into a very small *Apex*; then the Glass was plac'd on kindled Coals, which drove out fumes at the small orifice of the neck for a pretty while. Afterwards the Glass, being seal'd up at the *Apex*, was kept in the fire above two hours; and then being taken off was broken at the same *Apex*; whereupon I heard the outward Fire rush in, because when the Retort was seal'd the Air within it was highly rarified. Then the body of the Glass being broken, the Tin was taken out, consisting of a Lump about which there appear'd some gray *Calx* and some very small globules, which seem'd to have been filings melted into that form. The whole weigh'd two Ounces twelve Grains, the later part of which weight appear'd to have been gain'd by the Operation of the Fire on the Metal.

Additional Experiments. 47

Metal. In the neck of the Retort, where it was joyn'd to the body, there appear'd a yellowish and clammy substance thinly spread, which smelt almost like the foetid Oyl of Tartar.]

EXPER. VI.

To vary the foregoing Experiments by making Tryals on a Mineral that is held to be of a very Metalline nature, but is not a true Metal, nor will be brought to fusion by so moderate a Heat as will suffice to melt Tin, and yet has parts less fixt than Tin, as being far more easily sublimable, we thought fit to make the following Experiment:

[We took an Ounce of filings of Zinke carefully weigh'd, and having as carefully put them into a round Bolt-glass, we caus'd the neck to be drawn out very slender, and then order'd the Laborant to keep it upon quick Coals for the appointed time. Afterwards returning home, I call'd for the Glass, which he said he had

48 **Additional Experiments.**

kept four hours upon the Coals; answering me also, that there did for a great part of the time Smoak appear to ascend from the *Zink* and get out at the unstopt *Apex*. And in effect I observ'd, that the upper part of the Glass was lin'd with *Flores* or Sublimate of a darkish gray. The Glass being dextrously cut asunder, we took out not only the filings of *Zinke*, some of which were melted into little globuls, but the *Flores* too, and yet weighing all these in the same Scales, we had us'd before, we found five Grains and somewhat better wanting of an Ounce. Which we the less wonder'd at, because of the continuance of the lately mention'd Exhalations emitted by the filed Mineral.]

EXPER. VII.

For more ample confirmation of the truth discover'd by what I have been reciting about Tin, I thought fit to try the like Experiment upon another Metal; which though of some-

Additional Experiments. 49

somewhat more difficult fusion than Tin, I had reason to think might, if employed in a moderate quantity, and warily managed, be kept melted in Glass without breaking it. And accordingly having carefully weigh'd out four Ounces of good Lead cut before-hand into pieces little enough for the orifice of the Glass, I caused them to be put into a small Retort with a long neck, wherein was afterwards left but an orifice not much bigger than a pins head: Then leaving directions with the Laborant what to do, because I was my self call'd abroad, at my return he brought me together with the Glass, this Account: That he had kept it over and upon the Coals two hours, or better, and then supposing the danger of breaking the Glass was over, he had sealed it up at the little Orifice newly mention'd, and kept it on the Coals two hours longer. Before the Glass (which found to be well seal'd) was broken, perceived the pieces of Lead to have been melted into a Lump, whose

50 Additional Experiments.

surface was dark and rugged, and part of the Metal to have been turn'd into a dark-colour'd Powder or *Calx*: All this being taken out of the Retort, was weigh'd in the same Balance, whereon the Lead appear'd to have gain'd by the Operation somewhat above thirteen Grains.

EXPER. VIII.

To shew that Metals are not the only Bodies that are capable of receiving an increase of Weight from the Fire, I thought fit to make upon Coral a tryal, whereof my Memorial gives me this Account,

[Little bits of good red Coral being Hermetically seal'd up in a thin bubble of Glass, after two Drachms of them had been weigh'd out in a pair of nice Scales, were warily kept at several times over and upon kindled Coals, and at length being taken out for good and all, were found of a very dark Colour, and to have gain'd in weight three Grains and about a half.]

EXPER.

EXPER. IX.

One Experiment there is, which, though it might have come in more properly at another place, is not to be omitted in this; because it may invite us to consider, whether in the foregoing Experiments, excepting those made on Lead and Tin in seal'd Vessels, there may not be more of the Fire adherent to or incorporated with the Body expos'd to it, than one would conclude barely from the recited Increments of their Weight. For having taken very strong fresh Quick-lime provided on purpose for choice Experiments, and expos'd it, before the Air had time to slake it, upon the Cupel, to a strong fire where it was kept for two hours; I found that it had increas'd in weight even somewhat beyond my expectation: For being reasonably put into the Ballance, the Lumps that weigh'd, when expos'd, but two Drachms, amounted to two

I 4

Drachms

Drachms and twenty-nine Grains; which makes this Experiment a pregnant one to our purpose. For by this it appears, that notwithstanding a Body may for many hours, or even for some days, be expos'd to a very violent Fire; yet it may be still capable of admitting and retaining fresh Corpuscles; so that, though well made Lime be usually observ'd to be much lighter than the Stones whereof 'tis made; yet this lightness does not necessarily prove, that, because a burnt Lime-stone has lost much of its matter by the Fire, it has therefore acquir'd no matter *from* the Fire; but only inferrs, that it has lost far more than it has got. And this may give ground to suspect, that in most of the foregoing tryals the accession of the fiery Particles was greater (though in some more, in others less so,) than the Ballance discover'd; since, for ought we know, divers of the less fixt Particles of the expos'd Body might be driven away by the vehemence of the Heat; and

con-

Additional Experiments. 53

consequently the Igneous Corpuscles that fastned themselves to the remaining matter might be numerous enough, not only to bring the accession of Weight that was found by the Scales, but to make amends for all the fugitive Particles, that had been expell'd by the violence of the Fire. And since so fixt a Body as Quick-lime is capable of being wrought upon by the Igneous Effluvia, so as that they come to be as were incorporated with it, it may perchance be worth considering, whether in other calcin'd or incinerated bodies the remaining *Calces* or Ashes may not retain more than the bare impression (unless that be stretch'd to mean some participation of a substance,) of the Fire. Whether these particles that adhere to or are mixed with the stony ones of the Lime may have any thing to do in the heat and tumult that is produc'd upon the slaking of Lime, this is not fit place to examine. And *though* this Experiment and those made in

54 Additional Experiments.

in seal'd Retorts, which shew that what is afforded by Fire may in a Corporeal way invade, adhere and add Weight to even fixt and ponderous Bodies, there is a large Field open'd for the Speculative to apply this Discovery to divers *Phænomena* of Nature and Chymistry; yet I shall leave this Subject unmedled with in this place.

A
DISCOVERY
Of the
PERVIOUSNESS
OF
GLASS
TO PONDERABLE PARTS
OF
FLAME.

With some Reflexions on it by way
of COROLLARY.

*Subjoyned as an Appendix to his Experiments
about Arresting and Weighing of IGNEOUS
CORPUSCLES,*

BY
The Honorable ROBERT BOYLE.

LONDON:

Printed by W. G. for M. Pitt at the sign of the
White Hart, over-against the little North
Door of St Paul's Church. 1673.

A
DISCOVERY
OF THE
PERVALENCE
OF
GLASS
TO PONDERRABLE PARTS
OF
FLAME.

With some Reflections on it by way
of COROLLARY.

By the Author of an Appendix to his Experiments
about Attraction and Weights of Liquors
COROLLARY.

BY
The Honorable ROBERT BOYLE.

LONDON:
Printed by W. B. for M. P. in the Year of the
Whigs 1691. Over against the little Theatre
Door of St. James's Church 1691.



A DISCOVERY

The Perviousness of GLASS

T O
Ponderable Parts of FLAME.

THAT I might obviate some needless scruples that may be entertain'd by suspicious Vits upon this Circumstance of our additional Experiments, *That the Glasses employ'd about them were not exposed to the Action of mere Flame, but were held upon Charcoals, (which to me may seem to contain but a Gross kind of Fire :.) And that also I might, by diversifying the way of trial, render such Experiments both more fit to afford Corollaries, and more serviceable to my other purposes, I attempted to make it succeed with a Body so thin and disingaged from*

from gross matter as mere Flame is allowed to be, knowing, that by going cautiously with it to work one might handle a Retort without breaking it, in spite of a violent agitation of kindled matter.

EXPER. I.

Supposing then that good common Sulphur by reason of its great Inflammability and the vehemence and penetrancy of its Flame, would be a very fit fuel for my purpose I provided a small double Vessel contrived, *that* the one should contain as many Coals as was necessary to keep the Sulphur melted, and *that* the other, which was much smaller and shap'd like a Pan, should contain the Brimstone requisite for our Tryal and (lastly,) *that* these two should be with a convenient Lute so joyned to one another, that all being closed at the top, save the orifice of the little Pan, (the fire and smoak of the Coals having their vent another way

Verbiouſneſs of GLASS, &c. 59

way,) no fire ſhould come at the Retort to be employed, but the flame of the burning Brimſtone. Then two ounces of filings of Tin being heedfully weigh'd out, and put into a Glaſs-Retort provided for ſuch Tryals, and made fit to be eaſily ſeal'd up at the neck, when the time ſhould be convenient, the Sulphur (which ought to be of the purer ſort) was kindled, and the Glaſs by degrees expoſed to it; where it continued, as the Laborant inform'd me, (the ſmell of Brimſtone, peculiarly offenſive to me, forbidding me to be preſent,) near two hours before the Metal melted; after which he kept the Retort near an hour and half more with the Metal melted in it. Then bringing it me to look upon, I perceived a pretty deal of darkiſh *Calx* at the bottom, and partly too upon the ſurface of the far greater part of the Metal, which now lay in one Lump. The part of the Retort that had been ſeal'd being broken off, we firſt took out the *Calx*, and then
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the Lump, and putting them into the Scales, they had been formerly weigh'd in, found them to have made a very manifest acquist of weight, which, if both the Laborant and I be not mistaken, (for the paper, which should inform us, is now missing) amounted to four grains and a half, gained by the recited Operation. Afterwards, we being grown more expert in making such Tryals, the experiment was repeated with the same quantity of filings of the same Metal: At the end of the Operation, (which in all lasted somewhat above three hours) having broken off the seal'd neck of the Retort, we found, that a good proportion of dark-colour'd *Calx* had been produc'd. This being weighed with the uncalcin'd part of the Metal, the two ounces we first put in appear'd to have acquir'd no less than eleven grains and a half (and somewhat better.)

Such Superstructures, both for number and weight, may possibly
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Verbidusness of GLASS, &c. 61

in time be built on this and the like Experiments, that I shall venture to obviate even such a scruple as is like to be judg'd too Sceptical. But I remember, that, considering upon occasion of some of the Experiments formerly recited, that though it were very improbable, yet it did not appear impossible, that the increment of Weight, acquir'd by Bodies expos'd in Glass-vessels to the Fire, might proceed, not from the Corpuscles of Fire, but from the Particles of the Glass it self, loosened by the power of so intense a Heat, and forcibly driven into the inclos'd Body; I was content to take a couple of Glasses, whereof one was shap'd into a little Retort, and having weigh'd them; and then having kept them for a considerable time upon kindled Coals, and then weigh'd them again, I could gather little of certainty from the Experiment, (the Retort at one time seeming to have acquir'd above half a grain in the fire,) save that there was no likelihood at all, that

so considerable an increase of weight, as we divers times obtain'd in close vessels, should proceed from the Glass it self, and not from the Fire.

E X P E R. II.

Because it seems evident enough, that, whatever Chymists tell us of their Hypostatical Sulphur, common Brimstone is a body Heterogeneous enough, having in it some parts of an oily or inflammable nature, and others acid, and very near of kin to the Spirits of Vitriol; I thought fit to vary our Experiment, by making it with a liquor that is generally reputed to be as Homogeneous as Chymists themselves are wont to render any, I mean with a Spirit of Wine, or some such liquor as will totally flame away without affording Soot, or leaving any drop of Phlegm behind it. In prosecution of this design, we carefully weighed out an ounce of filings of Block-Tin, and put them into a Glass-Retort, fit for
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Perbiousness of GLASS, &c. 63

the purpose, whose neck was afterwards drawn out to a great slenderness; and we also provided a conveniently shap'd metalline Lamp, such as that the flame of this ardent Spirit might commodiously burn in it, and yet not melt nor crack it; which Lamp, though furnished with a Cotton wick, afforded no Soot; because as long as it was supplied with liquor enough, it remained unburnt. These things being in readiness, the Retort was warily approach'd to the flame, and the Metal was thereby in a short time melted. After which the Glass being kept expos'd to the same flame for near two hours in all, the seal'd *apex* of the Retort was broken off, and here appear'd to have been produc'd a not inconsiderable Quantity of *Calx*, that lay loose about the remaining part of the Tin, which, upon its growing cold, was harden'd into a Lump. This, and the *Calx*, being taken out of the Retort with care, that no little fragment of Glass

should at all impose upon us ; was weigh'd in the same Scales as formerly , and found to have gain'd four grains and a half , besides the Dust that stuck in the inside of the Retort , of which we reckon'd enough to make about half a grain more ; so that of so fine and pure a flame as of this totally ardent Spirit , enough to amount to five grains was arrested , and in good measure fixt by its operation on the Tin it had wrought upon.

E X P E R. III.

For confirmation of the former tryal , wherein we had imployed the *Spiritus ardens* of Sugar , we made the like experiment with highly Rectified Spirit of Wine , only substituting an ounce of Lead instead of one of Tin. The event , in short , was this ; that after the Metal had been for two hours or better kept in the flame , the seal'd neck of the Retort being broken off , the external
Air

Verbiouſneſs of GLASS, &c. 65

Air ruſh'd in with a noiſe, (which ſhew'd the Veſſel to have been very tight,) and we found pretty ſtore of the Lead; for 'twas above ſeven ſcruples, turn'd into a grayiſh *Calx*, which together with the reſt of the Metal being weigh'd again, there was very near, if not full, fix grains of increaſe of weight acquir'd by the Operation.

1. *N. B.* The Lump of Lead, that remain'd after the newly recited Operation, being ſeparated from the *Calx*, was weighed and cut in pieces, that it might be put into a freſh Retort, wherein it was again expoſ'd to the flame of Spirit of Wine, that I might ſatiſfie my ſelf, whether probably the whole Body of the Lead might not, by repeated Operations, or (perhaps by one continued long enough) be reduc'd to *Calx*. And though, after the Retort (whoſe neck had been drawn out) had been kept in the flame for about two hours, it was, by the negligence of a Foot-boy, unluckily broken, and ſome of

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the

the *Calx* lost ; yet we made a shift to save about five grains of it, (whose colour was yellowish ;) which was enough to make it likely , that , if we had had conveniency to pursue the Operation to the utmost, the whole Metal might have been calcin'd by the action of the flaming Spirit.

2. *N.B.* And lest you should be induc'd by some Chymical conceits to imagine , that the particles that once belong'd to flame , did make more than a Coalition with those of the Lead , and by a perfect Union were Really transmuted into the Metal whose weight they increas'd ; I shall add , that (according to a Method elsewhere deliver'd) I examin'd the *seven scruples* of *Calx*, mention'd to have been made in the third Experiment , by weighing them in Air and Water , and thereby found, as I expected, that though the *absolute* Gravity of the Metal had been increas'd by the particles of Flame that stuck fast to it, yet this Aggregate

gate of Lead and extinguiſh'd Flame had loſt much of its *ſpecifick* Gravity. For, whereas Lead is wont to be to Water of the ſame bulk, as about *eleven* and a *half* to *one*, this ſubtil *Calx* of Lead was to Water of the ſame bulk little, if at all, more than as *nine* to *one*.

Theſe are not the only Experiments I made of the Operation of meer Flame upon Bodies inclos'd in Glaſſes; but theſe, I ſuppoſe, are ſufficient to allow me to comply with my preſent haſte, and yet make good the *Title* prefixt to this Paper. For, whence can this increaſe of *absolute* weight (for I ſpeak not of *ſpecifick* Gravity,) obſerv'd by us in the Metals expos'd to the mere flame, be deduc'd, but from ſome ponderable parts of that Flame? And how could thoſe parts invade thoſe of the Metal inclos'd in a Glaſs, otherwiſe than by paſſing through the pores of that Glaſs? But, becauſe I judge it unphilosophical, *either* to be more careful that what one writes ſhould *appear* ſtrange, than *be* true;

or to be forward to advance the repute of Strangeness, to the prejudice of the Interest of Truth, though it be perhaps but a remote one, or a collateral one; I shall deal so impartially, as to subjoyn on this occasion two or three short Intimations, that may prove *both* seasonable for Caution, in reference to the Porousness of Glass, *and* give a hint or two in relation to other Things.

I do not then by the foregoing Experiments pretend to make out the Porosity of Glass any farther, than is exprest in the *Title* of this Paper; namely, in reference to some of the Ponderable parts of Flame. For otherwise I am not at all of their mind, that think Glass is easily penetrable, *either*, as many do, by Chymical Liquors; *or*, as some, by Quicksilver; *or*, as others, at least by our Air: Those opinions not agreeing with the Experiments I made purposely to examine them, as you may find in another Paper.

Again, if we compare the Increase
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Perviousness of GLASS, &c. 69

we observe to be made in the Weight of the Bodies that we expose to the naked Fire, and those of the same or the like kinds that we included in Glasses, or so much as in Crucibles; it may be worth considering, Whether this difference in acquir'd weight may not give cause to suspect, that the Corpuscles, whereof Fire and Flame consists, are not all of the same size, and equally agitated, but that the interpos'd Vessel keeps out the grosser Particles like a kind of Strainer, though it gives passage to the minutest and most active?

I offer it also to Consideration, Whether this perviousness of Glass, even to the minute particles that pervade it, and their adhesion to the Metal they work on, does necessarily imply Pores *constantly* great enough to transmit such Corpuscles? or, Whether it may not be said, *that* Glass is generally of a closer Texture, than when in our Experiments the pores are open'd by the vehement Heat of the flame that beats upon it, and

and in that state may let pass Corpuscles too big to permeate Glass in its ordinary state ; and *that* this penetration is much assisted by the vehement agitation of the Igneous parts, which by the rapidness of their motion both force themselves a passage through the narrow pores of the Glass , and pierce deep enough into those of the included Body to stick fast there ; (as hail-shot thrown with ones hand against a board, will pass off from it , but being shot out of a Gun will pierce it , and lodge themselves in it ?) And I know a *Menstruum* that does not work upon a certain Metal whilst the liquor is cold , or but faintly heated , and yet by intending the Heat would be made to turn it into a powder or *Calx* , (for it does not properly dissolve it.)

Perhaps it may not be amiss to add on this occasion , that though Glass be generally acknowledged to have far smaller pores , than any other matter wont to be implied to make vessels , that are to be expos'd

Perviousness of GLASS, &c. 71

to the fire; yet till I be farther satisfy'd, I shall forbear *both* to determine, whether the rectitude, that some Philosophers suppose in the pores of Glass, as 'tis a transparent body, or rather in their ranks or rows, may facilitate the Perviousness we above observ'd in Glass, *and* to conclude from the foregoing Experiments, that ponderable parts of Flame will be able as well to pass through the pores of Metalline vessels as those of Glass. For though, with a silver vessel, made merely of plate without Soder, I made two or three Tryals (of which you may command an account) in order to the resolving of these doubts, yet by an accident, which, though it were not a surprizing one, was unlucky enough to defeat my endeavours, I was kept, for want of fit Accommodations, from bringing my intended tryals to an issue.

And now having endeavour'd by the foregoing Advertisements to prevent the having unsafe Consequences drawn

drawn from our Experiments; it remains that I briefly point at three or four *Corollaries* that may more warily be deduc'd from them. To which, if I get time, I may subjoin a hint or two about further Inquiries.

COROLLARY I.

Confirming this PARADOX, That Flame may act as a Menstruum, and make Coalitions with the Bodies it works on.

THE Experiments, we have made and recited of the permeating of Flame (as to some of its parts) through Glass-vessels, and of its working on included Metals, may much confirm the Paradox I have elsewhere propos'd, That Flame may be a *Menstruum*, and work on some Bodies at the rate of being so; I mean not only by making a notable Communion and Dissipation of the parts, but

but by a Coalition of its own particles with those of the fretted Body, and thereby *permanently* adding Substance and Weight to them. Nor is it repugnant to Flames, being a *Menstruum*, that in our experiment the Lead and Tin, expos'd to it, were but reduc'd to powder, and not dissolv'd in the form of a Liquor, and kept in that state. For, besides that the interpos'd Glass hinder'd the Ignitious particles from getting through in plenty enough; I consider, that 'tis not necessary, that all *Menstruums* should be such Solvents, as the objection supposes. For *whether* it be (as I have sometimes suspected,) that *Menstruums*, that we think simple, may be compounded of very differing parts, whereof one may precipitate what is dissolved by the other; or for some other Cause, I have not now time to discuss. Certain it is, that some *Menstruums* corrode Metals and other Bodies without keeping dissolved all, or perhaps any considerable part; as may be seen, if you put Tin
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in a certain quantity of *Aqua fortis*, which will in a very short time reduce it almost totally to a very white substance, which, when dry, is a kind of *Calx*. And so by a due proportion of Oyl of Vitriol, abstracted from Quicksilver by a strong fire, we have divers times reduc'd the main body of the Mercury into a white powder, whereof but an inconsiderable part would be dissoluble in water. And such a white *Calx* I have had by the action of another fretting Liquor on a Body not Metalline.

And having thus clear'd our Paradox of the oppos'd Difficulty, my haste would immediately carry me on to the next Corollary, were it not, that there is one *Phenomenon* belonging to this place that deserves to be taken notice of. For, *whether* it be, as seems probable, from the vehement agitation of the permeating particles of Flame, that violently tear asunder the Metalline Corpuscles, or from the nature of the Igneous *Menstruum*, (which being as 'twere percolated through

through Glas it self, must be strangely minute,) 'tis worth observing, how small a proportion, in point of weight, of the additional adhering Body may serve to corrode a Metal, in comparison of the Quantity of vulgar *Mentruums* that is requisite for that purpose. For, whereas we are oblig'd to employ, to the making the solution of crude Lead, several times its weight of Spirit of Vinegar, and (though not so many times) even of *Aqua fortis*, 'twas observ'd in our Experiment, that, though the Lead was increas'd but six grains in weight, yet above a score of it were fretted into powder, so that the *Corrosive* Body appear'd to be but about the twentieth part of the *corroded*.

COROLL. II.

*Proposing a PARADOX about
Calcination and Calces.*

Another Consequence, deducible
from our discovery of the perviousness
of

of Glas to Flame, may be this; That there is cause to question the Truth of what is generally taken for granted about Calcination, and particularly of the notion, that not only others, but Chymists themselves, have entertain'd about the *Calces* of Metals and Minerals. For, whereas 'tis commonly suppos'd, that in Calcination the greater part of the Body is driven away, and only the Earth, to which Chymists add the Fixt Salt, remains behind; and whereas even Mechanical Philosophers, (for two or three of Them have taken notice of Calcination,) are of opinion, that much is driven away by the violence of the fire, and the remaining parts by being depriv'd of their more radical and fixt moisture are turn'd into dry and brittle particles: Whereas these Notions, *I say*, are entertain'd about Calcination, it seems, that they are not well fram'd, and do not universally hold; since, at least they are not applicable to the Metals, our Experiments were made on. For, it *does not* appear
by

by our Tryals; that any proportion, worth regarding, of moist and fugitive parts was expell'd in the Calcination; but it *does* appear very plainly, that by this Operation the Metals gain'd more weight than they lost; so that the main body of the Metal remain'd intire, and was far from being, *either* as a *Peripatetick* would think, Elementary Earth, or a compound of Earth and Fixt Salt, as Chymists commonly suppose the *Calx* of Lead to be. From which very erroneous *Hypothesis* they are wont to infer the sweet Vitriol of Lead, which they call *Saccharum Saturni*, to be but the sweet Salt of it *extracted* only by the Spirit of Vinegar; which does *indeed* plentifully enough concur to *compose* it. Whence I conclude, that the *Calx* of a Metal even made (as they speak) *per se*, that is, by fire without aditament, may be, at least in some cases, not the *Caput mortuum*, or *Terra damnata*, but a Magistery of it. For, in the sense of the most intelligible of the Chymical Writers, that is

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properly a Magistery wherein the Principles are not separated, but the bulk of the Body being preserved, it acquires a new and convenient form by the addition of the *Menstruum* or Solvent employed about the preparation. And, not here to borrow any Argument from my Notes about particular Qualities, you may guess how true it is, that the greatest part of the Body, or all the radical moisture is expell'd in Calcination, which therefore turns the Metal into an arid unfusible powder; by this, That I have several times from *Calx* of Lead reduc'd corporal Lead. And I remember, that having taken what I guess'd to be but about a third or fourth part of the *Calx* of Lead, produc'd by the third Experiment; found by a tryal purposely devis'd that without any Flux-powder or any additament, but meerly by the application of the Flame of highly Rectified Spirit of Wine, there could in a short time be obtain'd a considerable proportion of malleable Lead whereof

whereof the part I had the Curiosity to examine, was true malleable Lead; so little was the arid powder, whence this was reduc'd, depriv'd by the foregoing Calcination of the suppo'sd radical moisture requisite to a Metal. The Consideration of what may be drawn from this Reduction in reference to the Doctrine of Qualities belongs not to this place.

C O R O L L. III.

One use, among the rest, we may make, by way of Corollary, of the foregoing Discovery, which is in reference to a Controversie warmly agitated among the Corpuscular Philosophers themselves. For; some of them, that follow the *Epicurean* or *Atomical Hypothesis*, think, that when Bodies are expos'd in close vessels to the fire, though the Igneous Corpuscles do not stay with the Bodies they invade, yet they really get through the Pores of the interpos'd Vessels, and permeate the included

L 2 Bodies

Bodies in their passage upwards; whereas others, especially favourers of the *Cartesian* Doctrine, will not allow the *Atomists* Igneous Corpuscles, which they take to be but vehemently agitated particles of Terrestrial matter, to penetrate such minute pores as those of Glass; but do suppose the operation of the fire to be perform'd by the vehement agitation made of the small parts of the Glass, and by them propagated to the included Bodies, whose particles by this violent Commotion are notably alter'd, and receive new Textures, or other modifications.

But our Experiments inform us, that, though neither of the two Opinions seems fit to be despised, yet neither seems to have hit the very mark; though the *Epicurean Hypothesis* comprize somewhat more of the Truth than the other. For, though it be not improbable, that the brisk agitation communicated by the small parts of the Glass to those of the Body contain'd in it, may contribute much

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to the effect of the fire; and though, by the small increment of weight, we found in our expos'd Metal, 'tis very likely, that far the greater part of the Flame was excluded by the close Texture of the Glass; yet on the other side 'tis plain, that Igneous particles were trajected through the Glass, which agrees with the *Epicureans*; and they, on the other side, mistook, in thinking that they did but pass through, and divide and agitate the included Bodies; to which nevertheless our Experiments shew, that enough of them, to be manifestly ponderable, did permanently adhere.

Whether these Igneous Corpuscles do stick after the like manner to the parts of meat, drest by the help of the fire, and especially roast-meat, which is more immediately expos'd to the action of the fire, may be a question, which I shall now leave undiscuss'd, because I think it difficult to be determin'd, though otherwise it seems worthy to be consider'd, in regard it may concern mens Health,

to know, *whether* the Coction of meat be made by the fire, only as 'tis a very hot body, *or* whether it permanently communicates any thing of its substance to the meat expos'd to it: In which (last) case it may be suspected, that not only the degree and manner of application of a fire, but the nature of its fuel may be fit to be consider'd.

C O R O L L. IV.

The Experiments above recited give us this further Information, That Bodies very spirituous, fugitive, and minute, may, by being associated with congruous particles, though of quite another nature, so change their former Qualities, as to be arrested, by a solid and ponderous Body, to that degree, as not to be driven away from it by a fire intense enough to melt and calcine Metals.

For, the foregoing Tryals (taking
 ? Exp. III. N.B. 2. in what I * lately deliver'd of the lessen'd
 specifick Gravity of calcin'd Lead)
 seems

seems plainly enough to discover, that *even* the agitated parts of flame, minute enough to pass through the pores of Glafs it self, were as 'twere entangled among the metalline particles of Tin and Lead, and thereby brought to be fixt enough to endure the Heat that kept those Metals in fusion, and little by little reduc'd them into *calces*: Which is a *Phænomenon* that one would not easily look for, especially considering how simple a Texture that of Lead or Tin may be suppos'd to be in comparison of the more elaborate structures of very many other Bodies. And this *Phænomenon*, which shews us, what light and fugitive particles of matter may permanently concur to the Composition of Bodies ponderous and fixt enough, may perchance afford useful hints to the Speculative; especially if this strict Combination of spirituous and fugitive substance with such, as being gross or unwieldy, are less fit than organiz'd matter to entangle or detain them, be applied, (as it

may be with advantage) to those aggregates of spirituous Corpuscles, and organical Parts , that make up the Bodies of Plants and Animals. And this hint may suggest a main Inference to be drawn from the Operations of the Sun-beams on appropriated subjects , supposing it to prove like that of flame on Tin and Lead.

And now having dispatch'd our *COROLLARIES*, we might here inquire, Whether all the particles of Fire and Flame, that are subtil and agitated enough to penetrate Glass, and fasten themselves to included Bodies, be reduc'd by Ignition to the same nature, or else retain somewhat of their proper Qualities? Which Inquiry I have some cause not to think so undeterminable, as at first blush it may appear. For, *one of the ways*, that may be propos'd for this *Examen*, is already intimated at the close of the third Experiment, which shews, that we may compare the specifick Gravity of the *Calces* of the
same

same Metal, made in Glasses by the operation of Flames; whose fuels are of very differing Natures. And I said, *one of the ways*, because 'tis not the only way I could name, and have partly tryed. But though I might say more concerning Expedients of this kind, and could perhaps propound other Inquiries that may reasonably enough be grounded upon the hitherto recited *Phænomena* (and those of some other like tryals,) yet I must not unseasonably forget, that the pursuit of such Disquisitions would lead me much farther than I have now the leisure to follow it.

E R R A T A.

Pag. 44. l. 19. r. some Metals work; pag. 1. in the Discourse about the Determinate Nature of Effluvia, add the name of the Author, viz. By the Honorable ROBERT BOYLE.

F I N I S.

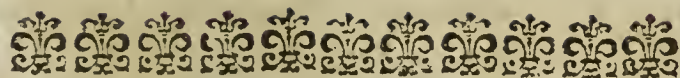
The Printer to the Reader.

IT hath been thought, it might be the Interest of the Reader, especially Foreigners, to be advertised, That these Essays are already Translating into Latin, and beginning also to be printed in that Language; which that it may duly be done, both as to this and the Author's other writings, to be publisht for the future, the greater care will be taken here, because it hath been several times found both at home and elsewhere, that the Versions made of them abroad, and not in the place, where in case of any difficulty the Author may be consulted with by the Latin Interpreters, are often very defective, and not seldom injurious to the sense he hath deliver'd them in. which being consider'd by those that desire to know the genuine sense of the Author, 'tis presumed, they will rather choose those Versions, which are made by persons that have that advantage of consulting him in any case of doubt, than
such

The Printer to the Reader.

such as shall mis-inform them ; notwithstanding the pretence of a cheaper rate of the Book.

which being thus advertised , the Printer taketh this opportunity of farther acquainting the Reader from the Latin Interpreter , that these Essays , to his knowledge , were ready and in the Press several Months before Dr. Thomas Bartholin's Acta Philosophica & Medica appear'd in England , in which there are two or three passages that may seem of affinity with some to be met with in the latter part of the Papers about Experiments of Arresting the parts of Flame , and of making them Ponderable.



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20. Se-

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20. *Several Tracts, containing New Experiments touching the Relation betwixt Flame and Air, and about Explosions: An Hydrostatical Discourse answering some Objections of Dr. Henry More: An Hydrostatical Letter, dilucidating an Experiment about a way of weighing Water in Water: New Experiments of the Positive or Relative Levity of Bodies under Water: Of the Air's Spring on Bodies under Water: About the differing Pressure of Heavy Solids and Fluids.* London, for Rich. Davies, 1672. in 8^o.

21. *Essays, of the strange Subtily, the great Efficacy, and the Determinate Nature of Effluvioms. To which are annext, New Experiments to make Fire and Flame Ponderable; together with a Discovery of the Perviousness of Glass.* London, for Moses Pitt, 1673. in 8^o.

22. *A Dialogue concerning the Positive or Privative nature of Cold; by a Member of the R. Society: And a Discourse about the Saltness of the Sea; and another of a Statical Hygroscope; together with some Phænomena of the force of the Air's Moisture. To which is added a Paradox about the Natural and Praternatural State of Bodies, especially the Air.* London, for Rich. Davies, 1673. in 8^o.

